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INORGANIC EVOLUTION AS STUDIED BY SPECTRUM ANALYSIS

by

Sir Joseph Norman Lockyer

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Truth may seem, but cannot be:
Beauty brag, but 'tis not she;
Truth and beauty buried be.

To this urn let those repair
That are either true or fair;
For these dead birds sigh a prayer.

Bacon

INORGANIC EVOLUTION AS STUDIED BY SPECTRUM ANALYSIS.

INORGANIC EVOLUTION

AS STUDIED BY

SPECTRUM ANALYSIS.

BY

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London
MACMILLAN AND CO., LIMITED

1900

PREFACE.

This present volume contains an account of my most recent inquiries into the chemistry of the stars, and of some questions which have grown out of these inquiries. It has taken its present form because several friends, upon whose judgment I can rely, suggested that I should preface the account of the work, and the conclusions I have derived from it, by a statement, as clear and simple as I could make it, of the principles of Spectrum Analysis and of the earlier steps in the various investigations the convergence of which has led to the present standpoint.

In my "Chemistry of the Sun," published in 1887, I dealt chiefly with the then state of the problem, so far as the Sun was concerned. In two later volumes, "The Meteoritic Hypothesis" and the "Sun's Place in Nature," I included the stars in the survey. The short story which I give in the earlier portion of the present book consists of a *résumé* of the three volumes, so far as the question of dissociation is concerned: this is followed by evidence recently accumulated by other inquirers, all of which tends to strengthen my original thesis. In the latter part of the volume I endeavour to show how, in the studies concerning dissociation, we have really been collecting facts concerning the evolution of the chemical elements; and I point out especially that the first steps in this evolution may possibly be best studied by, and most clearly represented in, the long chain of facts now at our disposal touching the spectral changes observed in the hottest stars.

My thanks are due (1) to Messrs. Lockyer, Fowler and Baxan-

a 2

dall, and other assistants at South Kensington, who have helped me to do the work; (2) to my colleagues, Professors Perry, Howes and Farmer, and Professor Poulton and Dr. Woodward, who have so freely given me information on several of the points touched upon in the later chapters; (3) to Professor Kayser, Sir William Crookes, Professor A. Schuster, and Dr. Preston, who have been good enough to look over the portions referring to their researches; and (4) to the officers and council of the Royal Society and to Messrs. Macmillan, for placing at my disposal some of the illustrations.

NORMAN LOCKYER.

Solar Physics Observatory,
South Kensington,
January 9, 1900.

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INORGANIC EVOLUTION AS STUDIED BY SPECTRUM ANALYSIS.

BOOK I.—THE BASIS OF THE INQUIRY.

CHAP. I.—PRINCIPLES AND METHODS.

THE thirty years' work to which I have to refer in this book has had to do with various points raised by the investigation of the radiation and absorption of light; the science of spectrum analysis is involved.

Spectrum analysis, indeed, is now becoming so far-reaching, especially in inquiries having to do with the conditions of the various celestial bodies, that there are many who are anxious to know something of its teachings. To some of these, however, the terms used by men of science, a very necessary shorthand, are unfamiliar, and appear hard to understand, because the opportunity of seeing the things they are intended to define, and which they generally do define in most admirable fashion, has never presented itself. I propose, therefore, to attempt to show that there is nothing recondite about these terms; that it is possible without any expensive apparatus for every one who will take a little trouble, to observe the phenomena for himself, after which the meanings of the terms employed will present no difficulty whatever.

One key to the hieroglyphics, the light story, which is hidden in every ray of light, is supplied to us by the rainbow. It teaches us that the white light with which nature bountifully supplies us in the sun's rays is composed of rays of different kinds or of different colours; and it is common knowledge that there is an almost perfect analogy between these coloured lights and sounds of different pitches.

The blue of the rainbow may be likened to the higher notes of the key-board of a piano, and the red of the rainbow, on the other hand, may be likened to the longer sound waves which produce the lower notes; and as we are able in the language of music to define each particular note, such as B flat and G sharp, and so on, so light-waves are defined by their colours or wave-lengths.

What nature accomplishes by a rain-drop we can do with a prism or a grating. A prism is a piece of glass or other transparent material through which the light is bent out of its course or *refracted* in the process. A grating is a collection of wires, or scratches on glass or metal, equidistant, very near together, and all parallel. When light passes through, or is reflected by such a system, it is said to be *diffracted*, and one result that we are concerned in is very similar to that of passing light through a prism.

It is rapidly becoming a familiar fact to many that when a ray of white light is refracted by a prism or diffracted by a grating a band of colour similar to a rainbow is produced, and that this effect follows because white light is built up of light of every colour, each colour having its own special length of wave and degree of refrangibility. Our rainbow band is called a spectrum.

Such a glass prism or grating is the fundamental part of the instrument called the spectroscope, and the most complicated spectroscope which we can imagine simply utilises the part which the prism or grating plays in breaking up a beam of white light into its constituent parts from the red to the violet. Between these colours we get that string of orange, yellow, green, and blue which we are familiar with in the rainbow.

A Simple Spectroscope.

For sixpence any of us may make for ourselves an instrument which will serve many of the purposes of demonstrating some of the marvellously fertile fields of knowledge which have been recently opened up to us. From an optician we can buy a small prism for sixpence; get a piece of wood from 20 to 10 inches long (the distance of distinct vision), 1 inch broad, and ½ an inch thick. On one end glue a cork 2 inches high, at the other end fasten, by melting the bottom, a stump of a wax candle of such a height that the dark cone above the

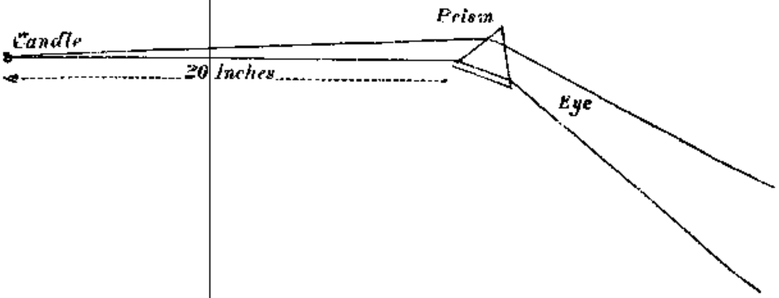

FIG. 1.—Arrangement of candle, prism and eye.

wick is level with the top of the cork. Then glue the prism on the cork, so that by looking sideways through the prism the coloured image or spectrum of the flame of the candle placed at the other end of the piece of wood can be seen.

We get a band of colour, a spectrum of the candle flame built up of an infinite number of images of the flame produced by the light rays of every colour. But, so far, the spectrum is impure, because the images overlap. We can get rid of this defect by replacing the candle by a needle.

If we now allow the needle to reflect the light of the candle flame, taking care that the direct light from the candle does not fall upon the face of the prism, we then get a much purer band of colour, because now we have an innumerable multitude of images of the thin needle instead of the broad flame close together. The needle is the equivalent of the slit of the more complicated spectroscopes used in laboratories.

FIG. 2.—Use of the simple spectroscope.

We can vary this experiment by gumming two pieces of tin foil with two perfectly straight edges on a piece of glass so that the straight edges are parallel and very near together. In this way we have a slit; this should be fixed close to the candle and between it and the prism.

Now the light of the candle is white, and the preceding experiment tells us that such light gives us a band containing all the colours without any breaks or gaps. We have what is called a *continuous spectrum.*

The Continuous Spectrum.

If we burn a piece of paper, or a match, or ordinary coal-gas, we get a white light identical to that given us by the candle; solids which do not liquefy when made white-hot, and liquids which do not volatilise under the same condition, and some dense gases when heated, do the same.

This effect is produced because there is light of every wave-length to produce an image of the needle (or the slit); these images blend together continuously from one end of the spectrum to the other.

Let us then consider this fact established, namely, that solid or liquid bodies and dense gases, when heated to a vivid incandescence, give a continuous spectrum. Under these circumstances the light to the eye, without the spectroscope, will be white, like that of a candle or white-hot poker.

The Length of the Continuous Spectrum varies with Temperature.

If we put a poker in a fire, it becomes red-hot; if we heat a platinum wire by passing a feeble current of electricity along it, it becomes red-hot like the poker.

In both cases examination by means of the prism shows that the red end only of the spectrum is visible. But if the poker or wire be gradually heated more strongly, the yellow, green, and blue rays will successively appear. Finally, when a brilliant white heat has been attained, the whole of the colours of the spectrum will be present.

Hence we learn that if the degree of incandescence be not high, the light will only be red. But, so far as the spectrum goes—and it will expand towards the violet as the incandescence increases, as before stated—it will be continuous.

The red condition comes from the *absence* of blue light; the white condition comes from the gradual addition of blue as the temperature increases.

One of the laws formulated by Kirchhoff in the infancy of spectroscopic inquiry has to do with the kind of radiation given out by bodies at different temperatures. The law affirms that the hotter a mass of matter is the further its spectrum extends into the ultra-violet.

Gaslight is redder than the light of an incandescent lamp because the latter is hotter. The carbons in a so-called arc-lamp give out a bluish-white light because they are hotter still.

By similar reasoning from experiment we are bound to consider the bluish-white stars, the white stars, the yellow, red and blood-red stars to indicate a decreasing order of temperature.*

We shall not go far wrong in supposing that the star with the most intense continuous radiation in the ultra-violet is the hottest, independently of absorbing conditions, which, in the absence of evidence to the contrary, we must assume to follow the same law in all.

An inquiry into the facts placed at our disposal by stellar photographs, shows that there is a considerable variation in the distance to which the radiation extends in the ultra-violet, and that the stars can be arranged in order of temperature on this basis.

Judged by this criterion alone, some of the hottest stars so far observed are γ Orionis, ζ Orionis, α Virginis, γ Pegasi, η Ursæ Majoris, and λ Tauri. Of stars of lower, but not much lower, temperature than the above, may be named Rigel, ζ Tauri, α Andromedæ, β Persei, α Pegasi, and β Tauri.

In this way spectrum analysis helps us with regard to *temperatures*, both on the earth and in the heavens.

Discontinuous Spectra with Bright Lines.

Let us next pass from a solid which retains its incandescence like platinum wire without melting, or a liquid which retains its incandescence, like molten iron, without volatilising and see what happens. We have found that when the light entering the slit consists of every colour and every tone, we have a *continuous* band of colour. If there is any defect in the light we must have a *discontinuous* one, for the reason that an image of the slit cannot be produced in any particular part of the spectrum if there be no light of that particular colour to produce it.

There are many artificial flames which are coloured, and if their light is analysed in the same way as the light of the candle, a perfectly new set of phenomena present themselves.

Let us again make use of our improvised spectroscope, and allow the needle to be illuminated by the flame of a spirit lamp into which salt is gradually allowed to fall; we see at once why the flame is yellow. It contains no red, green, blue, or violet rays, so that we should not represent the spectrum by

---

* On this point I wrote as follows in 1892: "An erroneous idea with regard to the indications of the temperature of the stars has been held by those who have not considered the matter specially. It has been imagined that the presence of the series of hydrogen lines in the ultra-violet was of itself sufficient evidence of a very high temperature. The experiments of Cornu, however, have shown that the complete series of lines can be seen with an ordinary spark without jar. Hence the high temperature of such a star as Sirius is not indicated by the fact that its spectrum shows the whole series of hydrogen lines, *but by the fact that there is bright continuous radiation far in the ultra-violet.*

VIBGYOR

as in the case of the candle, but simply by

Y

We see one image of the needle coloured in yellow.

We have passed from the spectrum of polychromatic to that of monochromatic light – from white light to coloured light – from light of all wave-lengths to light of one wave-length; from an infinite number of slit images giving a continuous band of every colour, to one image of the slit produced by light of one refrangibility, the colour of the image depending upon the refrangibility. What we shall see in passing from the spectrum of the candle to that of sodium vapour in the spirit lamp is shown in the accompanying woodcut.

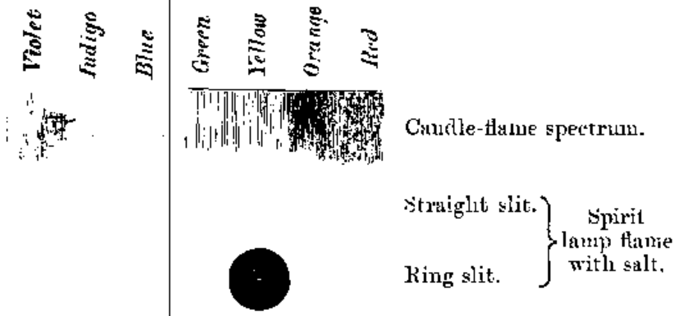

FIG. 3.—A continuous and a discontinuous spectrum.

That we are truly dealing with an image of the needle (or a slit) can be proved by using a slit of any shape. This can be shown by slightly altering our needle experiment. Take a piece of glass and a piece of tin-foil $1\frac{1}{2}$ inches square, cut out of the centre of the tin-foil a disc slightly larger than a threepenny-piece, and gum the remainder on the glass. In the centre, where the disc has been cut away, gum a threepenny-piece. The interval between the threepenny-piece and the tin-foil constitutes a circular slit. Let it replace the needle, and examine the flame of the spirit lamp charged with salt through it with the prism as before.

It will readily be grasped, from what has been stated, that in the case of coloured flames, the light passing through the spectroscope being only red, or yellow, or green, as the case may be, will go to build up an image of the slit in the appropriate part of the spectrum, and that the image thus built up will take the form of a line or circle, according to the slit we use.

Many chemical substances, salts of various metals, become luminous by inserting them into flames, as we have treated common salt (chloride of sodium). With each metal the colour imparted to the flame is different. The resulting spectrum is called a *discontinuous* spectrum, because it is only here and there that images of the slit are produced; because some coloured rays, and not all, are present.

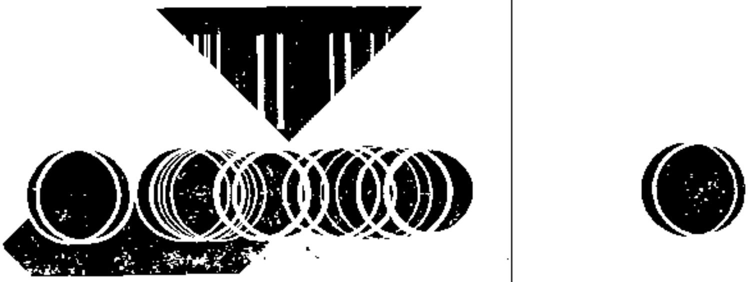

FIG. 4.—The spectrum of a complicated light-source as seen with a circular and a line slit.

The usual laboratory arrangement for observing the spectra of flames, is shown in the woodcut (Fig. 5).

Further, the system of images of the needle (or slit) varies for each substance, and it is on this ground that the term *spectrum analysis* is used, because we can in this way recognise the various substances in the flame.

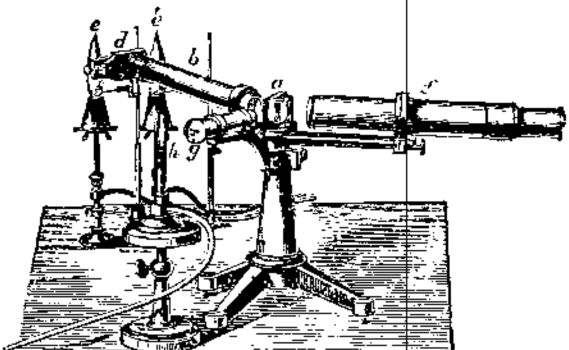

FIG. 5.—Observation of a flame spectrum with ordinary spectroscope with comparison prism. *a*, prism; *b*, collimator; *d*, slit; *e é*, flames to be compared; *f*, observing telescope; *g*, scale illuminated by *h* and reflected by the second surface of the prism into the telescope.

But we are not limited to flame temperatures; substances in a state of gas or vapour may be made to glow by electricity. At these higher temperatures very complicated spectra are produced, and again the spectrum is special to each chemical substance experimented on; the images of the needle (or slit), occupying different positions along the spectrum according to the nature of the source of light.

Fig. 5 gives us a laboratory prism spectroscope of small dispersion; with the more complicated spectra the phenomena are often better seen if more than one prism is employed. Fig. 6 shows an instrument in which four prisms are used.

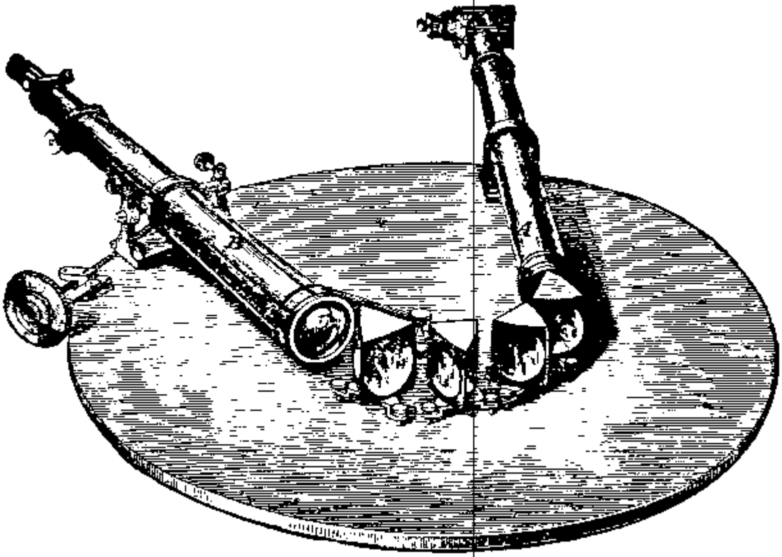

FIG. 6.—Steinheil spectroscope with four prisms.

It is in the case of the more complicated spectra that the wave-length has to be specially considered from the point of view of defining the position of a line. It is not enough to say, as was said in the case of the sodium line, that it is located in the orange.

The lengths of the various light-waves are very small. The wave-length of the sound-wave of the middle C of a piano is about 4 feet, while the wave-length of yellow light as defined by that of a line very accurately measured is ·0005895 of a millimetre, that is 5895 ten-millionths of a millimetre; so that there are 43,088 waves in a British inch. The unit of wave-length usually employed is the ten-millionth of a millimetre. These wave-lengths get shorter as we pass from the red to the violet.

For accurate measures of the wave-lengths of the lines a grating is employed as shown in Fig. 7.

So much then in general for the radiations given out by light sources, and the manner in which the spectroscope shows them and the student records their positions.

Spectrum analysis was established when experiment proved that no two substances which give a line spectrum give the same order of lines from one end of the spectrum to the other; in other words, the line spectrum of each chemical substance differs from that given by any other.

Here then is one of the secrets of the new power of investigation of which the spectroscope has put us in possession: we can recognise each element by its spectrum, whether that spectrum is produced in the laboratory or is given by light travelling earthwards from the most distant star, *provided the element exists both here and there.*

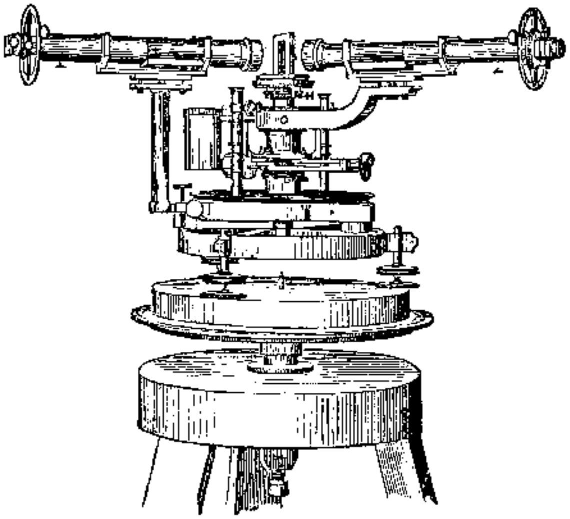

FIG. 7.—Angström's grating spectrometer.

It is in this way that spectrum analysis helps us with regard to

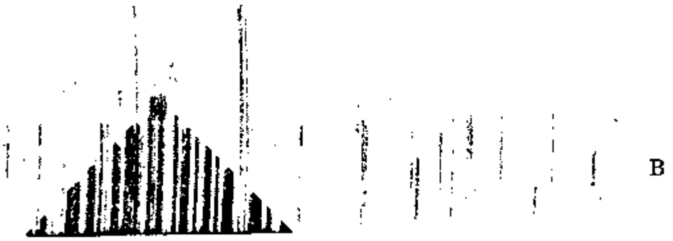

FIG. 8.—Parts of the spectra of (A) barium and (B) iron (from a photograph).

chemistry; the spectrum varies according to the chemical substance which produces it in a manner that will be gathered from an inspection of the photograph (Fig. 8) which shows the difference between the spectrum of barium (A), and that of iron (B).

Flutings.

The earliest spectroscope observations revealed the fact that in some spectra the lines, instead of being irregularly distributed along the spectrum, were arranged in an easily seen rhythmic fashion. Such allocations of lines are called flutings, as a succession of them gives rise to an appearance strongly recalling the flutings of a Corinthian column seen under a strong side light.

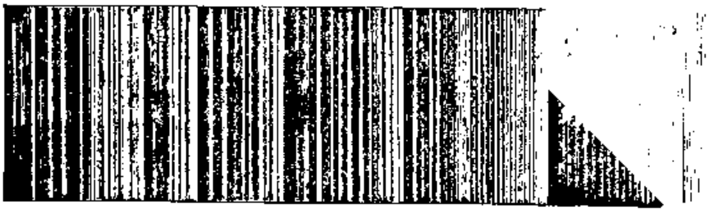

FIG. 9.—Fluting of carbon.

Our improvised spectroscope helps us here too; use the candle and straight slit in front of it as before, but shorten the slit, and only allow the blue light from the base of the candle flame to pass through it to the prism. We see two or three sets of flutings. These are the flutings of carbon; they are amongst the most beautiful examples known and are thoroughly typical.

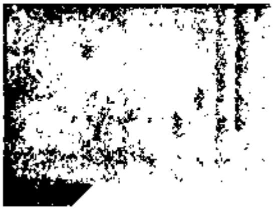

FIG. 10.—Fluting of magnesium.

Series.

One of the most important discoveries made in recent years, teaches us that in the case of many chemical elements, the apparently irregular distribution of the lines is really dominated by a most beautiful law, and that the most exquisite orderly rhythm can be obtained by sorting out the lines into what are termed "series," that is lines numerically related to each other.

Messrs. Runge and Paschen* showed, in 1890, that the spectra of lithium, sodium, and potassium were the summation of the spectra of various "series." Later they have shown that the same is true in the case of the cleveite gases.

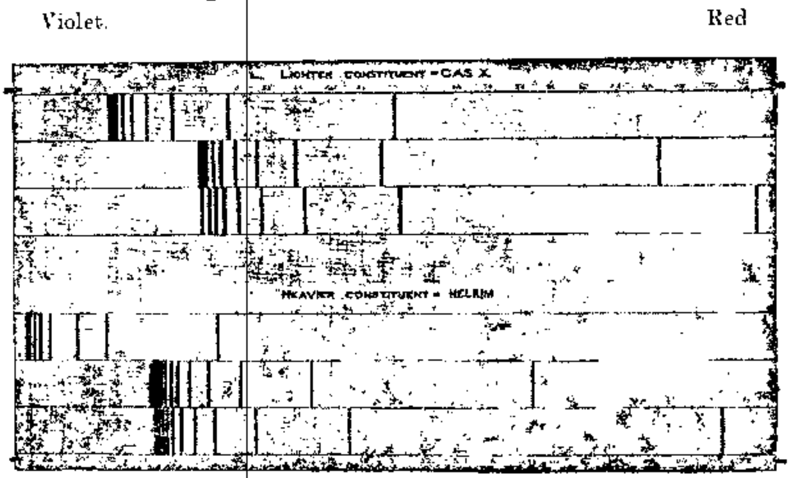

Fig. 11.—The series in the cleveite gases.

A "series" of spectral lines may be defined as a sequence of lines, the intensity of which decreases with the wave-length, and the wave-number or wave-frequency of which may be determined by the formula

$$A + B/n^2 + C/n^4,$$

where n represents the integers from three upwards, and the constants A, B, and C are determined for each element separately. The shorter the wave-lengths the greater number of waves there will be in a given length; hence the wave-frequency varies inversely as the wave-length.

The fact that lines must close up to one another, as the violet end of the spectrum is reached, indicates that the character of a "series" is best brought under notice in the ultra-violet end of the spectrum. In the visible part of the spectrum the lines forming "series" are too far apart to be recognised as belonging to a series.

The accompanying diagram (Fig. 11) shows how the apparently irregular lines observed in the spectra of the cleveite gases can be arranged into the most exquisite order when the six series of lines which build up the spectra are shown separately.

Some of these series are composed of triplets and some of doublets instead of single lines.

---

* *Abh. k. Akad. Wiss.*, Berlin, 1890.

I wrote thus on this subject in 1879:—

"I am at present engaged in investigating this question of rhythm, and I have already found that many of the first order lines of iron may probably arise from the superposition or integration of a number of rhythmical triplets. All this goes to show how long the series of simplifications is that we bring about in the case of the so-called elementary bodies by the application of a temperature that we cannot as yet define.

"Indeed, the more one studies spectra in detail, and especially under varying conditions of temperature which enable us to observe the reversal now of this set of lines, now of that, the more complex becomes the possible origin. Some spectra are full of doublets; others again are full of triplets, the wider member being sometimes on the more, sometimes on the less, refrangible side."*

Mascart† had noted this recurrence of similar features in spectra ten years earlier.

Discontinuous Spectra with Dark Lines.

It is time now to make still another experiment with our needle and prism.

If we study sunlight (taking care again to shield the prism), by allowing a sunbeam to illuminate the needle, we get a spectrum of a kind differing from those we have seen before, inasmuch as the continuous band of colour is broken, it is full of dark lines; that is, some of the coloured rays are lacking; and hence images of the needle are not forthcoming in places. The positions of some of the chief dark lines lettered by Fraunhofer are shown in Fig. 12.

We now know that this result is produced by what is termed the *absorption of light*. To understand it we have only to look at a candle through glasses of different colours: a blue glass absorbs or stops the red light, and only the blue end of the spectrum remains; a red glass absorbs or stops the blue, and only the red end remains.

In these cases large regions of the spectrum are alternately blotted out as differently coloured glasses are used, but the absorption with which we have to do mostly is of a more restricted character: lines, that is, single images of the slit, are in question.

One of the most important results that has been gathered from the study of these absorption effects is that if we look at a light source competent to give us a continuous spectrum through any of the vapours or gases we have so far considered as producing bright lines, provided the light source is hotter than the gases or vapours, the particular rays constituting the bright line or discontinuous spectrum of each of the vapours as gases will be cut out from the light of the continuous spectrum.

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* *Proc. Roy. Soc.*, vol. xxviii, March, 1879.

† In 1869, he wrote as follows: "Il semble difficile que la reproduction d'un pareil phénomène soit un effet du hasard: n'est-il pas plus naturel d'admettre que ces groupes de raies semblables sont des harmoniques qui tiennent à la constitution moléculaire du gaz lumineux? Il faudra sans doute un grand nombre d'observations analogues pour découvrir la loi qui régit ces harmoniques."

Explanation of Absorption.

While in the giving out of light we are dealing with molecular vibration taking place so energetically as to give rise to luminous radiation; absorption phenomena afford us evidence of this motion of the molecules when their vibrations are far less violent. The molecules can only vibrate each in its own period, and they will even take up vibrations from light which is passing among them, provided always that the light thus passing among them contains the proper vibrations.

An illustration from what happens in the case of sound will help to make this clear. If we go into a quiet room where there is a piano, and sing a note and stop suddenly, we find that note echoed back from the piano. If we sing another note, we find that it is also re-echoed from the piano. How is this? When we have sung a particular note, we have thrown the air into a particular state of vibration. One wire in the piano was competent to vibrate in harmony with it. It did so, and, vibrating after we had finished, kept on the note.

This principle may be illustrated in another and very striking manner by means of two large tuning-forks mounted on sounding-boxes and tuned in exact unison. One of the forks is set in active vibration by means of a fiddle-bow, and then brought near to the other one, the open mouths of the two sounding-boxes being presented to each other to make the effect as great as possible. After a few moments, if the fork originally sounded is damped to stop its sound, it will be found that the other fork has taken up the vibration and is sounding, not so loudly as the original fork was, but still distinctly. If the two forks are not in perfect unison, no amount of bowing of the one will have the slightest effect in producing sound from the other. Again, suppose we have a long room, and a fiddle at one end of it, and that between it and an observer at the other end of the room there is a screen of fiddles, all tuned like the solitary one, we can imagine that in that case the observer would scarcely hear the note produced upon any one of the open strings of the solitary fiddle. Why? The reason is that the air-pulses set up by the open string of this fiddle, in tune with all the others, would set all the other similar strings in vibration; the air pulses set in motion by the vibration of the fiddle cannot set all those strings vibrating and still pass on to one's ear at the other end of the room as if nothing had happened to them.

Now apply this to light. Suppose we have at one end of a room a vivid light source giving us all possible waves of light from red to violet. This we may represent as before by

V I B G Y O R

Also suppose that we have in the middle of the room a screen of molecules, say a sodium flame, capable of emitting yellow light,

Y

What will happen? Will the light come to our eyes exactly as if the molecules were not there? No; it will not. What then will be the difference? The molecules which vibrate at such a rate that they give out yellow light, keep for their own purpose—filch, so to speak, from the light passing through them—the particular vibrations which they want to carry on their own motions, and we shall have

V I B G   O R

as a result; the light comes to us minus the vibrations which have thus been utilised, as we may put it, by the screen of vapour. We have, in fact, an apparently dark space which may be represented thus:

V I B G **Y** O R

In the spectroscope we see what would otherwise be a continuous spectrum, with a dark band across the yellow absolutely identical in position with the bright band observed when the molecules of the vapour of which the screen is composed radiated light in the first instance. It is not, however, a case of absolute blackness, or absence of that particular ray, for the molecules are set in vibration by the rays which they absorb, and therefore give out some light, but it is so feeble as to appear black by contrast with the very much brighter rays coming direct from the original source.

This great law may be summed up as follows: *Gases and vapours, when relatively cool, absorb those rays which they themselves emit when incandescent;* the absorption is continuous or discontinuous (or selective) as the radiation is continuous or discontinuous (or selective).

I have referred to this matter at some length because in our light sources, in the sun, and in most of the stars we have light from a more highly heated centre passing through an envelope of cooler vapours, and on this account absorption phenomena are produced.

It was Fraunhofer, at the beginning of the century which is now so rapidly passing away, who was the discoverer of the fact that the spectrum of the sun was discontinuous with dark lines.

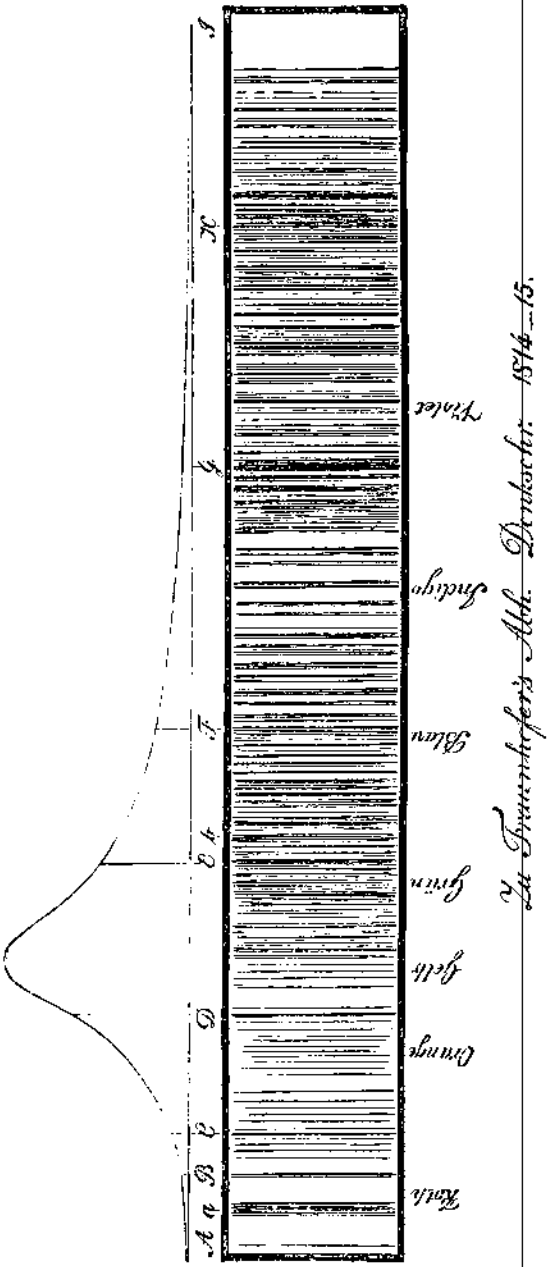


FIG. 12.—Copy of Fraunhofer's diagram of the lines in the solar spectrum with his original lettering of the principal lines.

When we wish to go further afield than the sun, that is, to the stars, we must first use a telescope to collect the light, and then employ a spectroscope.

Fig. 13 shows a spectroscope thus attached at the eye-piece end of the great Lick refractor. In astronomical inquiries the same methods of work are employed, and although it will be seen that we are now far beyond the improvised spectroscope with which we began, both in construction and use, no new principle is involved.

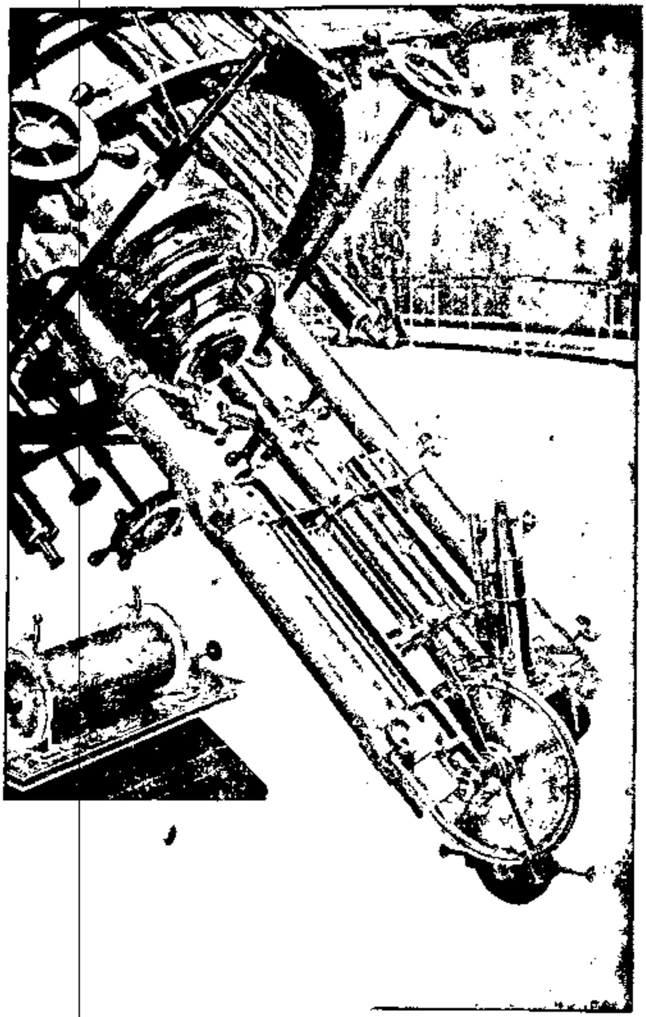


Fig. 13.—A stellar spectroscope attached to the Lick equatorial.

Now if my reader has not hesitated to invest his or her sixpence in a prism, and has had the patience (no other quality is needed) to do what I have suggested, the way is open to read without difficulty most books involving spectrum analysis which he or she is likely to come across; terms such as

Spectrum
Continuous spectrum
Grating
Prism
Spectroscope
Slit
Line spectra
Fluted spectra
Discontinuous (or selective) spectrum
Fraunhofer lines
Wave-length, wave-frequency
Radiation
Absorption
Series,

should now have acquired a definite meaning, and I trust the expressiveness of the terms will be acknowledged while they are accepted as part of the future mental stock-in-trade.

Chap. II.—Some Pioneering Difficulties.

When I began to endeavour to apply the principles of spectrum analysis to the investigation of the nature of the heavenly bodies in 1865, the then idea, based upon Kirchhoff and Bunsen's work of 1859, was that the spectrum of a chemical element was one and indivisible—that it could not be changed by temperature or by anything else.

Looking back it is easy to see now that this idea largely depended upon the fact that in the early days low-flame temperatures were generally employed, and that it so happens that the substances best visible in the flame and which were therefore chosen to experiment upon, such as sodium, calcium, potassium and the like, give us line spectra at low stages of heat.

Hence the first spectroscopic ideas entirely agreed with those of the chemist, that the chemical "atom," defined by a certain "atomic" weight was a manufactured article, indivisible, indestructible. Chemical elementary substances were either composed of these atoms, these indivisible units; or of "molecules" consisting of two or more of them, hence the terms "diatomic" and "polyatomic" molecule.

The difference between the spectra of the same element in the solid and gaseous states, in which we have first a continuous and secondly a line spectrum, was ascribed to the restricted motion of the atom in the solid and its freedom in the gaseous state—it was a question of "free path." The difference between the states which gave us the continuous and discontinuous spectra was a physical difference having nothing to do with chemistry. According to the kinetic theory of gases, the particles of all bodies are in a state of continual agitation, and the difference between the solid, liquid, and gaseous states of matter is that in a solid body the molecule never gets beyond a certain distance from its initial position. The path it describes is often within a very small region of space. Prof. Clifford, in a lecture upon atoms, many years ago illustrated this very clearly. He supposed a body in the middle of a room held by elastic bands to the ceiling and the floor, and in the same manner to each side of the room. Now pull the body from its place; it will vibrate, but always about a mean position; it will not travel bodily out of its place; it will always go back again.

We next come to liquids. Concerning these we read: "In fluids, on the other hand, there is no such restriction to the excursions of a molecule. It is true that the molecule generally can travel but a very small distance before its path is disturbed by an encounter with some other molecule; but after this encounter, there is nothing which determines the molecule rather to return towards the place from whence it came than to push its way into new regions. Hence in liquids the path of a molecule is not confined within a limited region, as in the case of solids, but may penetrate to any part of the space occupied by the liquid.

Now we have the motion of the molecule in the solid and the liquid. How about the movement in a gas? "A gaseous body is supposed to consist of a large number of molecules moving very rapidly." For instance, the molecules of air travel about 20 miles in a minute. "During the greater part of their course these molecules are not acted upon by any sensible force, and therefore move in straight lines with uniform velocity. When two molecules come within a certain distance of each other, a mutual action takes place between them which may be compared to the collision of two billiard balls. Each molecule has its course changed, and starts in a new path."

The collision between two molecules is defined as an "encounter"; the course of a molecule between encounters a "free path." "In ordinary gases the free motion of a molecule takes up much more time than is occupied by an encounter. As the density of the gas increases the free path diminishes."

It will be seen at once that on the view first held that the difference between continuous and discontinuous spectra depended simply upon the solid and gaseous states, no solid could give us a line spectrum; and the well-known absorption spectra of didymium glass and other solid bodies would be impossible.

Another important series of facts was soon brought to the front. Plücker and Hittorf in the year 1865, announced that "there is a certain number of elementary substances which when differently treated furnish two kinds of spectra of quite a different character, not having any line or band in common." The difference in character to which reference is here made consists in the spectrum produced at the lower temperature being composed of flutings, which are replaced by lines when the higher temperature is reached.

This was the first blow aimed at the general view—one element one spectrum—to which I have referred above. It was met in two ways.

Taking the line spectrum as representing the true vibration of the chemical unit, I have already shown that the continuous spectrum was explained as due to its physical environment, the solid or liquid state. This, then, had not to be considered from the chemical point of view.

The fluted spectra were boldly ascribed to "impurities," but not always wisely, for, to get rid of the difficulty presented by the two spectra of hydrogen, two perfectly distinct spectra were ascribed to acetylene. Again the "bell-hypothesis" was suggested, according to which the spectrum did not depend so much upon the substance as upon the way it was made to vibrate. According to this view the same chemical atom might have a dozen spectra if struck in a dozen different ways.

But it was answered that this argument proved too much; and for this reason. Mitscherlich showed in 1864 that some bodies known to be chemical compounds when raised to incandescence, give us a spectrum special to the compound; that is, they have a spectrum of their own; no lines of either of the constituents are seen.

I showed later that when the temperature was sufficient to produce decomposition, the lines of the elementary bodies of which the compound was composed made their appearances according to the temperature employed. And I also showed that precisely the same thing happens with regard to the fluted and line spectra of the same chemical element. We may get the first alone at a low temperature; we may increase the temperature and dim it slightly, some lines making their appearance; and next, by employing a very high temperature, we can abolish the fluted spectrum altogether and obtain one with lines only.

Since then the difference between the two spectra of the same element was no more marked than the difference between the spectrum of a known compound and its constituents after the compound had been broken up by heat, it was as logical to deny the existence of compound bodies as to deny that more molecular complexities than one were involved in spectral phenomena.

Attacks like these finally caused the chemists to reconsider their position, and some time later, being under the impression, which has turned out to have no justification, that "monatomic" elements like mercury have not fluted spectra, they conceded that the fluted spectra might represent the vibration of the "diatomic" molecule in the "diatomic" elements. This, of course, was to give up the "bell-hypothesis."

At the time when the differences of opinion arising from the existence of fluted as well as line spectra in the case of many elements were being discussed, solar observations were beginning to bring before us a perfect flood of facts apparently devoid of any law or order. In 1866 I threw an image of the sun on the slit of a spectroscope (Fig. 14), in order to observe the spectra of its different parts, and in this way the spectra of sun-spots (Fig. 19) and eventually of prominences were observed.

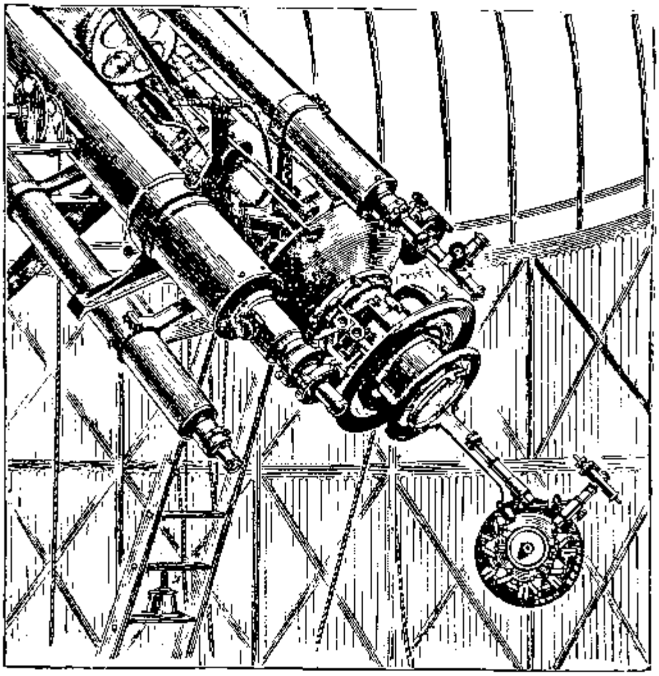
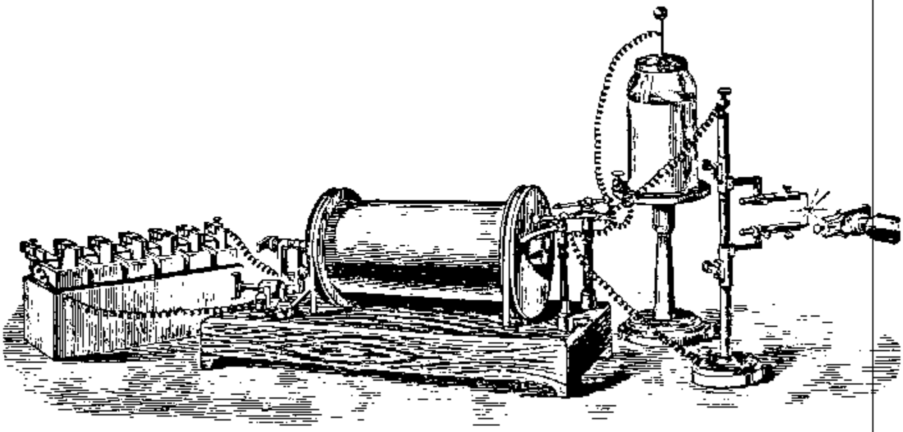


Fig. 14.—Spectroscope attached to a large refractor which throws an image of the sun on the slit plate.

In the first method of work adopted in the laboratory the spectroscope was directed to the light source, so that the spectrum was built up of the light coming from all parts of it without distinction.



IG. 15.—The first method of work with the slit of the spectroscope close to the light source. In the experiment illustrated the light source is an electric spark produced by an induction coil with Leyden jar in circuit. The slit end of the spectroscope is shown to the right.

In 1869 I introduced into laboratory work the method adopted in the case of the sun in the observatory; that is, an image of each light source experimented on was thrown on to the slit by a lens (Fig. 16), so that the spectrum of each part of it could be observed, and some of the results obtained by the new method were the following:—

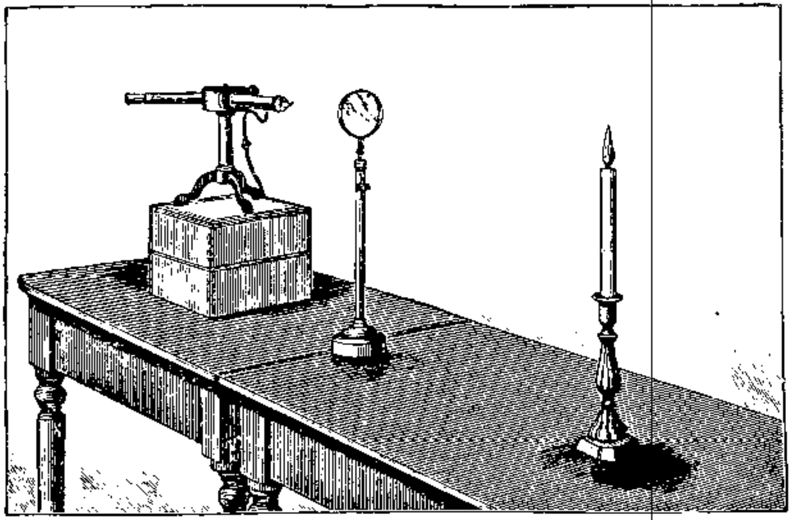


FIG. 16.—The method of throwing an image of the light source (in this case a candle flame) on the slit plate of a laboratory spectroscope.

The spectral lines obtained by using such a light source as the electric arc or spark were of different lengths; some appeared only in the spectrum of the centre of the light source, others extended far into the outer envelopes. This effect was best studied by throwing the image of a horizontal arc or spark on a vertical slit. The lengths of the lines photographed in the electric arc of many metallic elements, were tabulated and published in 1873 and 1874.

In Figs. 17 and 18 these so-called "long and short lines" are illustrated. In one case we deal with a mixture of the salts of calcium and strontium, in the other with the metal sodium. The richness of the lines in the spectrum of the core of the arc will be best gathered from Fig. 17, the variations in the lengths of the lines from Fig. 18.

Here then was the first glimpse of the idea that the complete spectrum of a chemical element obtained at the highest temperature might arise from the summation of two or more different line spectra, produced at different degrees of temperature, and therefore bringing us in presence of two or more molecular complexities; that is, different molecules broken up at different temperatures. So soon as experiments in the laboratory had given a definite result with regard to the spectrum of a metal in this way, I proceeded to study the sun with a view of determining how that metal behaved in the sun.

This involved, first, photographs of the solar spectrum with its dark lines, photographic comparisons of these dark lines with the bright lines constituting the spectra of the metallic elements. This enabled us to compare the total light given by each light source with the light received from all parts of the sun indiscriminately.

FIG. 17.—The long and short lines. Copy of a photograph taken with a vertical slit when compounds of strontium and calcium were volatilised between horizontal carbon poles.

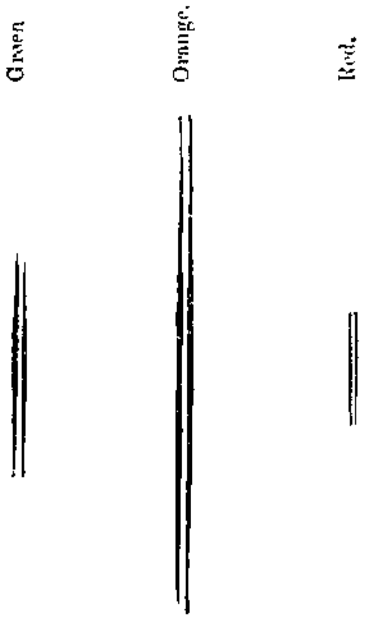


FIG. 18.—The longs and shorts of sodium taken under the same conditions, showing that the orange line extends furthest from the poles.

Next the spectra of different parts of the sun—chromosphere and prominences and spots—were compared with different parts of the light source, the core of the arc, and the centre of the spark, and the outer regions of both.

It will be seen that the inquiry now had a very broad base, and it could be immediately tested in many ways at every stage.

Wonderful anomalies were at once detected; lines known to belong to the same chemical element behaved differently in several ways. Some were limited to prominences, others to spots (Fig. 20), and in solar storms different iron lines indicated different velocities (Fig. 21). In the spectrum of the hottest part of the sun open to our inquiries, the region namely immediately overlying the photosphere, which I named the chromosphere, the anomalies became legion; suffice to say that in the hottest part of the sun we could get at, the spectrum of iron then represented in Kirchhoff's map of the ordinary solar spectrum by 460 lines was reduced to three lines.

It was no longer a question merely of settling the difficulties raised by the observations of Plücker and Hittorf.

Many observations and cross references of this kind during the next few years convinced me that the view that each chemical element had only one line spectrum was erroneous, and that the results obtained suggested that the various terrestrial and solar phenomena were produced by a series of simplifications brought about by each higher temperature employed. That is, that the new instrument, the spectroscope, showed that higher temperatures than those previously employed were doing for chemistry what previous similar inquiries had done, namely, indicating the existence of finer constituents in matter supposed at each point of time to be elementary.

This was the first glimpse of dissociation in relation to the production of changes in the line spectrum.

By the year 1872 the work of Rutherfurd and Secchi on stellar spectra enabled the base of the inquiry to include the stars as well as the sun. In some of the stars the existence of hydrogen, magnesium, and carbon were beyond question. The point that first struck me was that in white stars like α Lyræ and Sirius, with continuous spectra extending far into the violet—stars therefore hotter than their fellows of a yellow or red colour—we had to do with hydrogen almost alone.

It was in 1873 that I first called the attention of the Royal Society to the very remarkable facts which had even then been brought together regarding the possible action of heat in the sun and stars. Referring more especially to the classification of stars by Rutherfurd, I wrote as follows:—*

"I have asked myself whether all the above facts cannot be grouped together in a working hypothesis which assumes that in the reversing layers of the sun and stars various degrees of 'celestial dissociation" are at work, which dissociation prevents the coming together of the atoms which, at the temperature of the earth and at all artificial temperatures yet attained here, compose the metals, the metalloids and compounds."

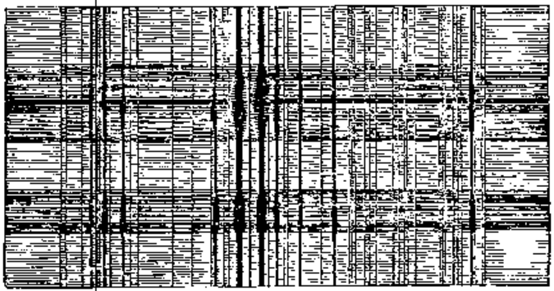


FIG. 19.—Spectrum of a sun-spot as compared with the general spectrum, showing that certain metallic lines (sodium and calcium in this instance) are widened. The darker portion represents the spectrum of the spot.

Subsequently in a private letter to M. Dumas, who took the keenest interest in my solar work, I wrote, "Il semble que plus une étoile est chaude plus son spectre est simple."

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* *Phil Trans.*, vol. clxiv, Part II, p. 491.

I also pointed out the close relation of hydrogen to calcium, magnesium and other metals (it was on this ground that I had named the substance which gave D^3, which always varied with hydrogen, helium), and the absence of all other terrestrial gases from the solar spectrum. An interesting discussion at the Paris Academy of Sciences was thus concluded by M. Dumas:

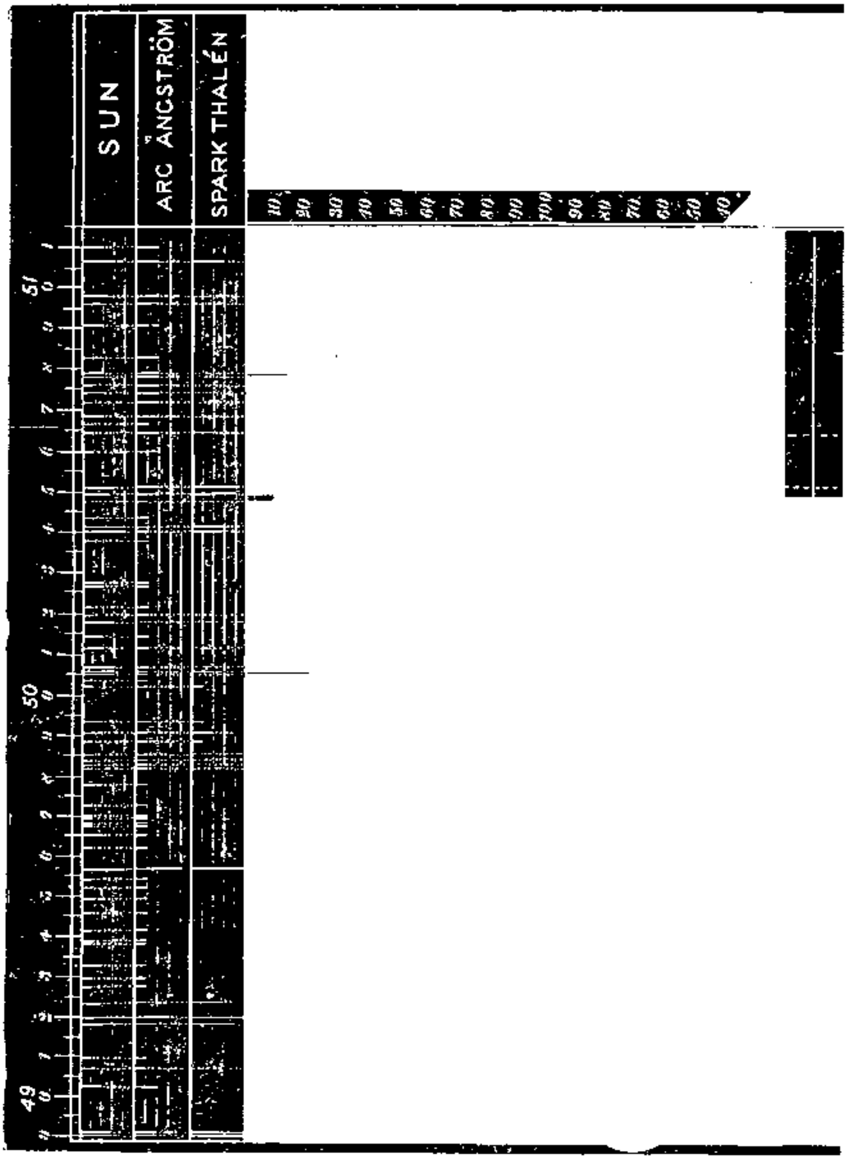

FIG. 20.—Iron spot lines at Kensington confronted with iron prominence lines at Palermo.

"En résumé, quand je soutenais devant l'Académie que les éléments de Lavoisier devaient être considérés, ainsi qu'il avait établi lui-même, non comme les éléments *absolus* de l'univers, mais comme les éléments *relatifs* de l'expérience humaine ; quand je professais, il y a longtemps, que *l'hydrogène* était plus près des métaux que de toute autre classe de corps ; j'émettais des opinions que les découvertes actuelles viennent confirmer et que je n'ai point à modifier aujourd'hui."*

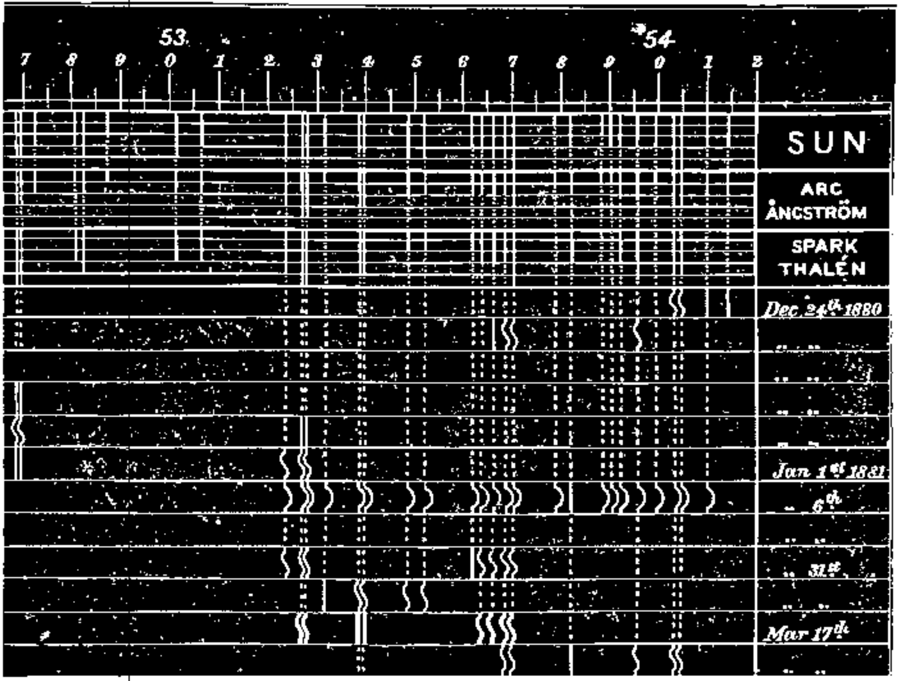

FIG. 21.—Different rates of motion registered by different iron lines.

One of the replies to my working hypothesis was that the various chemical elements probably existed in different proportions in the different stars, and that it so happened that in Vega and Sirius one of them, hydrogen, existed practically alone.

In 1878 I went further, and showed that thousands of solar phenomena which had been carefully recorded during the previous years could only be explained by assuming that the changes in the various intensities of lines in the line spectrum itself indicated successive dissociations. I pictured the effect of furnaces of different temperatures, and I wrote as follows:†

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* *Chemistry of the Sun*, p. 205.

† *Proc. Roy. Soc.*, vol. xxviii, p. 169. See also *Chemistry of the Sun*, chap. xviii.

"It is abundantly clear that if the so-called elements, or, more properly speaking, their finest atoms—those that give us line spectra—are really compounds, the compounds must have been formed at a very high temperature. It is easy to imagine that there may be no superior limit to temperature, and therefore no superior limit beyond which such combinations are possible, because the atoms which have the power of combining together at these transcendental stages of heat do not exist as such, or rather they exist combined with other atoms, like or unlike, at all lower temperatures. Hence association will be a combination of more complex molecules as temperature is reduced, and of dissociation, therefore, with increased temperature, there may be no end."

In 1878 I went back to the study of the changes in the line spectra in relation to the changes observed when known compounds were dissociated, and after discussing certain objections, I submitted the conclusion that the known facts with regard to the changes in line spectra "are easily grouped together, and a perfect continuity of phenomena established on the hypothesis of successive dissociations analogous to those observed in the cases of undoubted compounds."*

It is thus seen that the conclusions to which my spectroscopic work up to the year 1880 had led me, tended in exactly the same direction as that indicated by more purely chemical inquiries thus referred to by Berthelot in that year:—

"L'étude approfondie des propriétés physiques et chimiques des masses élémentaires, qui constituent nos corps simples actuels, tend chaque jour d'avantage à les assimiler, non à des atomes indivisibles, homogènes et susceptibles d'eprouver seulement des mouvements d'ensemble, . . . il est difficile d'imaginer un mot et une notion plus contraires à l'observation ; mais à des édifices fort complexes, doués d'une architecture spécifique et animés des mouvements intestins très variés."†

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* *Proc. Roy. Soc.*, vol. xxviii, p. 179.

† *Comptes rendus*, 1880, vol. xc, p. 1512.

CHAP. III.—THE PRESENT POSITION.

IN the last chapter I referred to some of the difficulties encountered by the earlier researchers in spectrum analysis. In the present one I propose to pass over the history of nearly twenty years' work, with all its attendant doubts and difficulties, and deal with what that work has brought us, a perfect harmony between laboratory, solar and stellar phenomena.

It has been proved beyond all question that not only are both fluted (or channelled-space) spectra and line spectra visible in the case of most of the elements, but that many of the metallic elements with which I shall have to deal in the sequel have at least two sets of lines accompanying, if not resulting from, the action of widely differing temperatures.

It is important to mention that the different chemical elements behave very differently in regard to the action of heat and electricity upon them as we pass from the solid to the liquid and vaporous forms; that is, the two different forms of energy are apt to behave very differently; the permanent gases as opposed to the elements which generally exist in the solid form is the first differentiation; the elements of low atomic weights and low melting point as opposed to the rest, is the second.

In the cases in which heat-energy can go so far, we first get an increase in the free path of the molecules, and ultimately the latter are made to vibrate.

In the case of high-tension electricity, on the other hand, increase of free path is scarcely involved, and hence we may have effects similar to those produced by high temperature, with scarcely perceptible effects of heat in the ordinary sense.

Conversing on this subject with my friend Clifford, many years ago, we came to the conclusion that the energy imparted to a molecule might cause (1) an extension of free path; (2) a rotation; and (3) a vibration. To get concrete images of these effects we spoke of *path-heat*, *spin-heat*, and *wobble-heat*. The facts seemed to show that heat energy had no effect in producing line-spectra until the two first results had been obtained, and, further, that in all gases and many metals it had no effect in producing vibrations; while, on the other hand, electrical energy generally acted as if it began at the third stage and is effective in the case of every chemical substance without exception.

However this may be, we now know that many elements present changes at several widely differing stages of heat. The line spectra of elements like sodium, lithium, and others may be obtained by the heat of the flame of a spirit lamp, or an ordinary Bunsen's burner, the substance being introduced into the flame by a clean platinum wire twisted into a loop at the end.

This temperature has no effect upon iron and similar metals. To get any special spectral indication from them a higher temperature than that of the Bunsen is required; the blowpipe flame may be resorted to; in this a stream of air is blown through the centre of a flame of coal gas burning at the end of a cylindrical tube.

We get in this way what is called a "flame-spectrum," in which flutings and some lines are seen. In order to obtain the complete line-spectra of some of the less volatile metals, like iron and copper, we are driven to use electrical energy and employ the voltaic current, and (for choice) metallic poles, which are so strongly heated by the passage of the current that the vapour of the metal thus experimented on is produced and rendered incandescent.

We may say generally that no amount of heat-energy will render visible the spectra of gases. These are obtained by enclosing the gases in glass tubes, and illuminating them by means of an electric current. We may go further and say that the ordinary voltaic current used in laboratories is equally inoperative. We must have the induced current, and with different tensions different spectra are produced.

We have then arrived so far. Heat-energy, which does give us line-spectra in some cases when metals are concerned, fails us in the case of the permanent gases and many metals. A voltaic current gives us spectra when metals are in question, but, like heat-energy, it will not set the particles of the permanent gases vibrating.

But when both metals and the permanent gases are subjected to the action of a strong induced current, that is, a current of high tension, when an induction coil with Leyden jars and an air break are employed, we get this vibration; gases now become luminous, a distinct change in the spectra of the metals is observed, a change as well marked, or perhaps better marked, than any of the previous lower temperature changes to which I have already drawn attention.

When the tension is still further increased, the differences in the spectra are most marked in the case of gases, for the reason that, being enclosed in tubes, they cannot escape from the action of the current; all the molecules are equally affected. *The spectrum is sometimes* NOT *a mixed one.* In the case of the metals the spark is made to pass between two small pointed poles, and the region of most intense action is a very limited one; we get from the particles outside this region the spectrum obtained with a lower degree of electrical energy. *The spectrum* is *a mixed one*. Even when we take the precaution of throwing an image of the spark on the slit of the spectroscope, the outer cooler layers pierced by the line of sight add their lines to the spectrum of the centre.

Not only so, but the individuality of the various chemical elements comes out in a remarkable manner.

To take one or two instances. I will begin with the gases with a weak and strong induced current. Hydrogen gives us what is termed a structure spectrum, a spectrum full of lines; this changes to a series. Oxygen gives us series which change into a complicated line spectrum in which no series has been traced. Nitrogen gives us a fluted spectrum, which changes into a complicated line-spectrum.

I next pass to the metals, and again, for brevity's sake, I will deal with three substances only. In the case of magnesium, iron, and calcium, the changes observed on passing from the temperature of the arc to that of the spark have been minutely observed. In each new lines are added, or old ones are intensified at the higher temperature. Such lines have been termed *enhanced lines*.

These enhanced lines are not seen alone; as in the case of the spark, so in the arc outside the region of high temperature in which they are produced, the cooling vapours give us the lines visible at a lower temperature.

Bearing in mind what happens in the case of the gases, we can conceive the enhanced lines to be seen alone at the highest temperature in a space sufficiently shielded from the action of all lower temperatures, but such a shielding is beyond our laboratory expedients; still, as I shall show, in the atmospheres of the stars we have probably the closest approximation open to our observation of that equally heated space condition to which I have referred.

The enhanced lines are very few in number as compared with those seen at the temperature of the arc. In the case of iron thousands are reduced to tens.

The above statements are only general; if we include the non-metals, more stages of temperature are required, and it then becomes evident that different kinds of spectra are produced at the same temperature in the case of different elements; in other words, at many different heat-levels changes occur, always in one direction, but differing widely for different substances at the lower temperatures. At the highest temperatures—at the limit—there is much greater constancy in the phenomena observed if we disregard the question of series. If considered from the series point of view, there is no constancy at all.

It is obvious that with all these temperature effects observed in a large number of elements, very many comparisons are rendered possible. All these suggest that if dissociation is really in question, in some cases at least more than two simplifications in the line stage are necessary to explain the facts. It is possible that the effects at first ascribed to quantity may be due to the presence of a series of molecules of different complexities, and that this is the true reason why "the more there is to dissociate, the more time is required to run through the series, and the better the first stages are seen."*

After this general statement of the changes in spectra observed to accompany change in the quantity and kind of energy used in the experiments, I propose to refer briefly to the most recent work on this subject, touching the changes observed on passing from the arc to the spark in the case of many of the metallic elements. By the kindness of Mr. Hugh Spottiswoode, the photographs of the enhanced lines have been obtained by the use of the large induction coil, giving a 40-inch spark, formerly belonging to Dr. Spottiswoode, P.R.S. I am anxious to express here my deep obligation to Mr. Hugh Spottiswoode for the loan of such a magnificent addition to my instrumental stock-in-trade.

The spark obtained by means of the Spottiswoode coil is so luminous that higher dispersions than those formerly employed can be effectively used, and in consequence of this, the detection of the enhanced lines becomes more easy; their number therefore has been considerably increased.

At the higher temperature enhanced lines have been found to make their appearance in the spectra of nearly all the metals already examined. Lithium is one exception.

Neglecting then all changes at the lowest temperatures, but including the flame spectrum, four distinct temperature stages are indicated by the varying spectra of the metals; for simplicity I limit myself to iron as an example. These are :—

1. The flame spectrum, consisting of a few lines and flutings only, including several well-marked lines, some of them arranged in triplets.

2. The arc spectrum, consisting, according to Rowland, of 2,000 lines or more.

3. The spark spectrum, differing from the arc spectrum in the enhancement of some of the short lines and the reduced relative brightness of others.

4. A spectrum consisting of a relatively very small number of lines which are intensified in the spark. This, as stated above, we can conceive to be visible alone at the highest temperature in a space efficiently shielded from the action of all lower ones, since the enhanced

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* *Proc. Roy. Soc.*, 1879, No. 200.

lines behave like those of a metal when a compound of a metal is broken up by the action of heat.

Each line of each element, at whatever temperature it is produced, can at once be compared in relation to position in the spectrum with the lines visible in celestial bodies with a view of determining whether the element exists in them.

At the time at which the earlier inquiries of this kind were made it was only possible for the most part to deal with eye observations of the heavenly bodies. The results were, therefore, limited to the visible spectrum.

During the last few years photographs of the spectra of the brighter stars and of the sun's chromosphere during eclipses have been obtained; it became of importance, therefore, to extend the observations of terrestrial spectra into the photographic regions for the purpose of making the comparisons which were necessary for continuing the inquiry.

The recent work has been done with this object in view.

The way in which the enhanced lines have been used is as follows. Those belonging to some of the chief metallic elements have been brought together, and thus form what I have termed a "test-spectrum." This has been treated as if it were the spectrum of an unknown element, and it has been compared with the various spectra presented by the sun and stars.

How marvellous, how even magnificent, the results of this inquiry have been, I shall show later in detail; but I may here say by way of anticipation that the test-spectrum turns out to be practically the spectrum of the chromosphere, that is, the spectrum of the hottest part of the sun that we can get at; and that a star has been found in which it exists almost alone, nearly all the lines of which had previously been regarded as "unknown."

This last result is of the highest order of importance, because it should carry conviction home to many who were not satisfied with the change of spectrum as seen in a laboratory, where always the enhanced lines seen in the spectrum of the centre of the spark have alongside them the lines in the spectrum of the outer envelope, which of course is cooling, and in which the finer molecules should reunite. For twenty years I have longed for an incandescent bottle in which to store what the centre of the spark produces. The stars have now provided it, as I shall show.

Although I have promised to pass over the history of the work generally, I must still point out that the enhanced lines in the test-spectrum actually include all those first studied years ago when everything was dim, and we were seeing through a glass darkly; not as we are now, face to face. To show the rigid connection of the new with the old, it is desirable to refer briefly to some of the work undertaken in relation to some of the first anomalies noted.

One advantage of this method of treatment is that it shows that the immense mass of facts now available supports all the conclusions drawn from the meagre evidence available a quarter of a century ago.

Some of the anomalies were as follows: they are given as specimens of many.

1. Inversion of intensity of lines seen under different circumstances.

I showed in 1879 that there was no connection whatever between the spectra of calcium, barium, iron and manganese and the chromosphere spectrum beyond certain coincidences of wave-length. The long lines seen in laboratory experiments are suppressed, and the feeble lines exalted in the spectrum of the chromosphere. In the Fraunhofer spectrum, the relative intensities of the lines are quite different from those of coincident lines in the chromosphere.

2. The simplification of the spectrum of a substance at the temperature of the chromosphere. To take an example, in the visible region of the spectrum, iron is represented by nearly a thousand Fraunhofer lines; in the chromosphere it has only two representatives.

3. In sun spots we deal with one set of iron lines, in the chromosphere with another.

4. At the maximum sun-spot period the lines widened in spot spectra are nearly all unknown; at the minimum they are chiefly due to iron and other familiar substances.

5. The up-rush or down-rush of the so-called iron vapour in the sun is not registered equally by all the iron lines, as it should be on the non-dissociation hypothesis. Thus, as I first observed in 1880, while motion is sometimes shown by the change of refrangibility of some lines attributed to iron, other adjacent iron lines indicate a state of absolute rest.

Laboratory work without stint has been brought to bear, with a view of attempting to explain the anomalies to which attention has been directed.

I only refer here to the work done on iron, magnesium and calcium, to show that in those metals the anomalies were to a large extent due to the lines now termed enhanced—that is, the lines seem to considerably change their intensities when the highest temperatures are employed.

Iron.

In the course of my early observations of the spectrum of the chromosphere, I discovered on June 6, 1869, a bright line at 1474 on

Kirchhoff's scale, which I stated to be coincident with a line of iron. On June 26 I discovered another at 2003·4 of the same scale.

The later researches on the spectrum of iron have shown that the iron line which I observed in 1869 to be coincident with the bright chromospheric line at 1474 on Kirchhoff's scale, having a wave-length of 5316·79, is an enhanced line, agreeing absolutely with Young's latest determination of the wave-length of the 1474 chromospheric line.

Similarly the line at 2003·4 of Kirchhoff's scale, with a wave-length of 4924, is also an enhanced line of iron.

The first experiments were made to explain my own and the Italian observations of the chromosphere which proved the presence of only these two lines of iron in the part of the spectrum ordinarily observed; the ordinary spectrum of iron, in which 460 lines had been mapped at that time, was entirely invisible.

The anomalies were investigated in the experimental work with sparks produced by quantity and intensity coils, with and without jars in the circuit. The outcome of these experiments was to show that the chromospheric representatives of iron were precisely the lines which were brightened on passing from the arc to the spark, while the lines widened in spots corresponded to a lower temperature.

The next anomaly observed was that in a sun spot the iron line at 4924 often indicated no movement of the iron vapour, while the other iron lines showed that it was moving with considerable velocity.

It seemed perfectly clear then that in the sun "we were not dealing with iron itself, but with primitive forms of matter contained in iron which are capable of withstanding the high temperature of the sun, after the iron observed as such has been broken up, as suggested by Brodie."*

On this view, the high temperature iron lines of the chromosphere represent the vibrations of one set of molecules, while the lines which are widened in spots correspond to other molecular vibrations. Similarly, the idea of different molecular groupings provides a satisfactory explanation of the varying rates of movement of iron vapour indicated by adjacent lines, the lines being produced by absorption of different molecules at different levels and at different temperatures.

Magnesium.

In 1879 I passed a spark through a flame charged with vapours of different substances. In the case of magnesium the effect of the higher temperature of the spark was very marked; some of the flame lines being abolished, while two new ones made their appearance, one of them at 4481. The important fact was that the lines special to the flame did not appear among the Fraunhofer lines, while some of those of the spark did appear.

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* *Proc. Roy. Soc.*, vol. xxxii, p. 204.

This line at 4481 now takes its place among the enhanced lines like those of iron previously mentioned; special cases now form part of the more general one.

Here again the experiments pointed to varying degrees of dissociation at different temperatures as the cause of the non-appearance of some of the magnesium lines in the Fraunhofer spectrum.

From these experiments, the results of which were subsequently mapped in relation to the various heat-levels indicated by solar phenomena, I drew the following conclusions in 1879:—

"I think it is not too much to hope that a careful study of such maps, showing the results already obtained, or to be obtained, at varying temperatures, controlled by observations of the conditions under which changes are brought about, will, if we accept the idea that various *dissociations* of the molecules present in the solid are brought about by different stages of heat, and then reverse the process, enable us to determine the mode of evolution by which the molecules vibrating in the atmospheres of the hottest stars *associate* into those of which the solid metal is composed. I put this suggestion forward with the greater confidence, because I see that help can be got from various converging lines of work."*

Calcium.

In 1876 I produced evidence that the working hypothesis that the molecular grouping of calcium which gives a spectrum having its principal line at 4226·9 is nearly broken up in the sun, and quite broken up in the spark, explained the facts which are that the low temperature line loses its importance in the spectrum of the sun, in which H and K are by far the strongest lines.

I summed up the facts regarding calcium as follows:—"We have the blue line differentiated from H and K by its thinness in the solar spectrum while they are thick, and by its thickness in the arc while they are thin. We have it again differentiated from them by its absence in solar storms in which they are almost universally seen, and, finally, by its absence during eclipses, while the H and K lines have been the brightest seen or photographed."

I afterwards attempted to carry the matter further by photographing the spectra of sun spots. In all cases H and K lines were seen reversed over the spots, just as Young saw them at Sherman, while the blue calcium line was not reversed. The oldest of these photographs which has been preserved bears the date April 1, 1881.

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* *Proc. Roy. Soc.*, 1879, vol. xxx, p. 30.

The experimental results in the case of calcium, therefore, followed suit with those obtained from iron and magnesium, and indicated that the cause of the inversion of intensities in the lines of a substance under different circumstances is due to the varying degrees of dissociation brought about by different temperatures.

Both in the case of iron, magnesium and calcium, the high temperature lines involved are not seen at all at lower temperatures, and even in the case of calcium, when photographic exposure of 100 hours' duration have been employed. It should be sufficiently obvious to everybody from this that temperature alone is in question.

Finally, then. The similar changes in the spectra of certain elements, changes observed in laboratory, sun and stars are simply and sufficiently explained on the hypothesis of dissociation. If we reject this, so far no other explanation is forthcoming which co-ordinates and harmonises the results obtained along the different lines of work. Nor is this all: as I shall show later on, there are other branches of physical inquiry which suggest the same hypothesis.

BOOK II.—APPLICATION OF THE INQUIRY TO THE SUN AND STARS.

Chap. IV.—The Sun's Chromosphere.

I stated in the previous chapter (p. 33), that in order to utilize the information placed at our disposal by the discovery of the new lines seen in the spectra of metals exposed to high temperatures, I had brought the enhanced lines of the chief metallic elements together, and thus formed a "test-spectrum" to use as a new engine of research in regions of work where help might be expected from it.

In this chapter I shall deal with the application of this test-spectrum to a study of the sun.

It is obvious that the general spectrum of the sun, like that of stars generally, is built up of all the absorptions *which can make themselves felt* in every layer of its atmosphere from bottom to top, that is from the photosphere to the outermost part of the corona. It is important to note that this spectrum is *changeless* from year to year.

Now sun spots are disturbances produced in the photosphere; and the chromosphere, with its disturbances, called prominences, lie directly above it. Here, then, we are dealing with the lowest part of the sun's atmosphere. We find first of all that in opposition to the changeless general spectrum, great changes occur with the sun-spot period, both in the spots and chromosphere.

The spot spectrum is indicated, as was found in 1866, by the widening of certain lines; the chromospheric spectrum, as was found in 1868, by the appearance at the sun's limb of certain bright lines. In both cases the lines affected seen at any one time are almost always relatively few in number.

Since 1868 we have been enabled to observe not only the spectrum of the sun's spots, but that of the chromosphere as well, every day when the sun shines. The chromosphere is full of marvels. At first, when our knowledge of spectra was very much more restricted than now, almost all the lines observed were unknown. In 1868 I saw a line in the yellow, which I found behaved very much like hydrogen, though I could prove that it was not due to hydrogen; for laboratory use the substance which gave rise to it I named helium. Next year, as I stated in the last chapter, I saw a line in the green at 1474 of Kirchhoff's scale. That was an unknown line, but in some subsequent researches I traced it to iron. From that day to this we have observed a large number of lines.

But useful as the method of observing the chromosphere without an eclipse, which enables us

> . . . to feel from world to world,"

as Tennyson has put it, has proved, we want an eclipse to see it face to face.

During the eclipses of 1893, 1896, and 1898, a tremendous flood of light has been thrown upon it by the use of large instruments constructed on a plan devised by Respighi and myself in 1871. These give us images of the chromosphere painted by each one of its radiations, so that the exact locus of each chemical layer is revealed. One of the instruments employed during the Indian eclipse has also been used in photographing metallic spectra and the spectra of stars, so that it is now easy to place photographs of the spectra of the chromosphere obtained during a total eclipse, and of the various metals and stars side by side.

As in the case of the photographs taken with the prismatic cameras in 1893 and 1896, the spectrum of the chromosphere in 1898 is very different from the Fraunhofer spectrum, so that we have not to deal with a mere reversal of the dark lines of ordinary sunlight into bright ones.

Many very strong chromospheric lines, the helium lines for example, are not represented among the Fraunhofer lines, while many Fraunhofer lines are absent from the chromospheric spectrum (Fig. 23).

But the most remarkable result is that in the eclipse photograph of the chromosphere spectrum, the most important of the metallic lines are precisely those included in the "test-spectrum" (Fig. 22). This photograph in fact deals chiefly with the enhanced metallic lines.

I recognise in this result a veritable Rosetta stone, which will enable us to read the terrestrial and celestial hieroglyphics presented to us in spectra, and help us to study them and get at results much more distinctly and certainly than ever before. The result proves conclusively that the absorption in the sun's atmosphere which produces the Fraunhofer lines is not produced by the hottest lowest stratum, the chromosphere.

It is imperative in order to clear the ground for the future study of stellar spectra, to inquire fully into the true *locus* of absorption. One of the most important conclusions we draw from the Indian eclipse is that, *for some reason or other*, the lowest hottest part of the sun's atmosphere does not write its record among the lines which build up the general spectrum so effectively as does another.

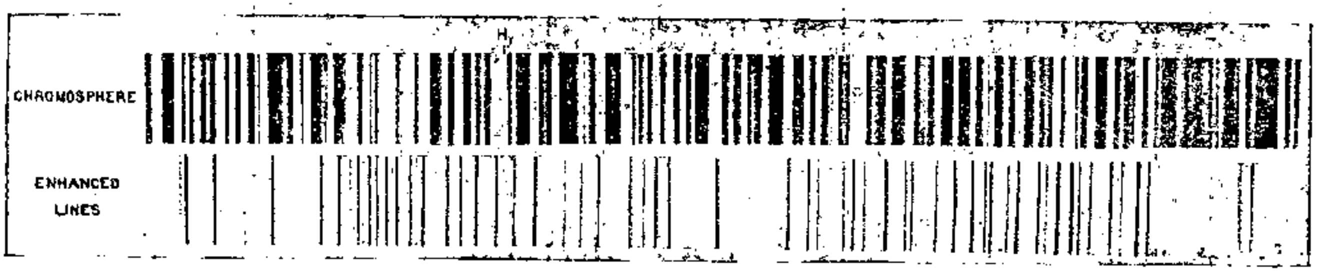

FIG. 22.—Comparison of the spectrum of the chromosphere with the test-spectrum.

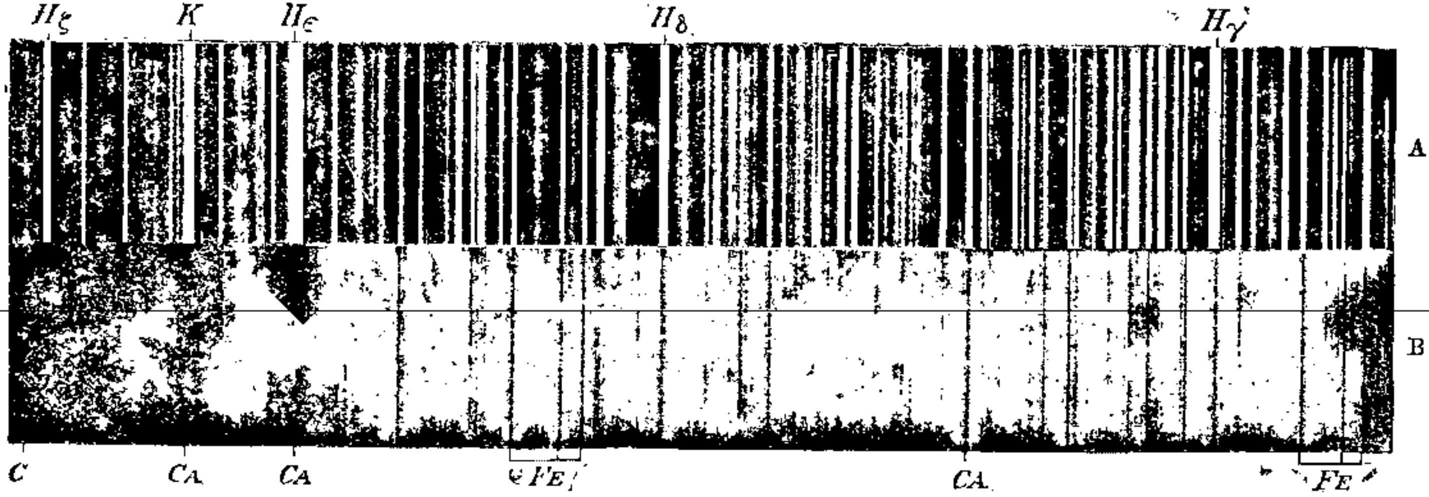

FIG. 23.—Spectrum of chromosphere A as photographed during eclipse, compared with Fraunhofer lines B.

This conclusion differs considerably from the opinion generally held. In my paper on the eclipse of 1893,* I referred at length to this point. The matter is so important that I do not hesitate to quote what I then said.

"As a result of solar spectroscopic observations, combined with laboratory work, Dr. Frankland and myself came to the conclusion, in 1869, that at least in one particular, Kirchhoff's theory of the solar constitution required modification. In that year we wrote as follows:—†

"'May not these facts indicate that the absorption to which the reversal of the spectrum and the Fraunhofer lines are due takes place in the photosphere itself, or extremely near to it, instead of in an extensive outer absorbing atmosphere?'

"In an early observation of a prominence on April 17th, 1870, I found hundreds of the Fraunhofer lines bright at the base, and remarked that 'a more convincing proof of the theory of the solar constitution put forward by Dr. Frankland and myself could scarcely have been furnished.'‡

"During the eclipse of 1870, at the moment of disappearance of the sun, a similar reversal of lines was noticed; we had, to quote Professor Young, 'a sudden reversal into brightness and colour of the countless dark lines of the spectrum at the commencement of totality.' On these observations was based the view that there was a region some 2″ high above the photosphere, which reversed for us *all* the lines visible in the solar spectrum; and on this ground the name 'reversing layer' was given to it.

"Continued observations, however, led me, in 1873, to abandon the view that the absorption phenomena of the solar spectrum are produced by any such thin stratum, and convinced me that the absorption took place at various levels above the photosphere. I need not give the evidence here; it is set forth in my *Chemistry of the Sun*.§ On the latter hypothesis the different vapours exist normally at different distances above the photosphere according to their powers of resisting the dissociating effects of heat.

"My observations during the eclipse of 1882, in the seven minutes preceding totality, to my mind set the matter at rest. 'We begin with one short and brilliant line constantly seen in prominences, never seen in spots. Next another line appears, also constantly seen in prominences; and now, for the first time, a *longer* and thinner line appears, occasionally noted as widened in spots; while, last of all, we get, very long, very delicate relatively, two lines constantly seen widened in spots, and another line, not seen in the spark, and never yet recorded as widened in spots.'*

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* *Phil. Trans.*, 1896, vol. clxxxvii, A, p. 603.

† *Proc. Roy. Soc.*, vol. xvii, p. 88.

‡ *Ibid.*, vol. xviii, p. 358.

§ Chapter XXII, pp. 303–309.

‖ *Proc. Roy. Soc.*, vol. xxxiv, p. 292.

"This is one of the most mportant points in solar physics, but there is not yet a concensus of opinion upon it. Professor Young and others, apparently, still hold to the view first announced by Dr. Frankland and myself in the infancy of the observations, that the Fraunhofer absorption takes place in a thin stratum, lying close to the photosphere."

I next proceeded to discuss the numerous photographs obtained during the eclipse, and I gave a map showing that there was only the slightest relation between the intensities of the lines common to the Fraunhofer and the eclipse spectrum, and further, that only a few of the Fraunhofer lines are represented at all. Not only this, but in the eclipse photographs there are many bright lines not represented at all among the Fraunhofer lines.

The chromosphere, which represents that part of the sun's atmosphere underlying the true reversing layer, is admirably pourtrayed in the photographs of the eclipse of 1898. So complete is the record that it is quite sufficient for our present purpose, and is the more to be relied on since it represents it at the same instant of time; I have elsewhere pointed out that Young's list of chromospheric lines may be misleading because it is a summation of results obtained at different times and of different conditions; prominences even may be, and doubtless are, involved. The lengths and intensities of the lines are faithfully recorded in the photographs.

An examination of the eclipse photographs shows that the temperature of the most luminous vapours at the sun's limb is not far from that produced by an electric spark of very high tension, the lines, which we have seen to be enhanced on passing from the arc to such a spark, being present.

The chromosphere, then, is certainly not the origin of the Fraunhofer lines, either as regards intensity or number. From the eye observations made since 1868, there is ample evidence that the quiescent chromosphere spectrum indicates a higher temperature than that at which much of the most valid absorption takes place; in other words, the majority of the lines associated with lower temperature are produced above the level of the chromosphere, and hence the true reversing layer, instead of being at the bottom of the chromosphere, as held by some, is really above it.

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* *Proc. Roy. Soc.*, vol. xxxiv, p. 297.

The eclipse photographs, however, at the same time afford evidence by the relative lengths of some of the lower temperature lines that we need not locate the region which produces the absorption indicated by the Fraunhofer lines at any great height above the chromosphere.

I may say that for some time I was of opinion that in the sun many of the darkest lines indicated absorptions high up in the atmosphere, for the reason that the bright continuous spectrum of the lower levels might have an important effect upon line absorption phenomena by superposing radiation, and so diminishing the initial absorption. The observations of the eclipses of 1893, 1896 and 1898, however, indicate that this opinion is probably only strictly true when the strata of the sun's atmosphere close above the photosphere are considered.

Let us next turn to the highest regions of the solar surroundings to see if we can get any effective help from them.

In this matter we are dependent absolutely upon eclipses, and certainly the phenomena observable when the so-called corona is visible, full of awe and grandeur to all, are also full of precious teaching to the student of science. The corona varies like the spots and prominences with the sun-spot period.

It happened that I was the only person that saw both the eclipse of 1871 at the maximum of the sun-spot period and that of 1878 at minimum; the corona of 1871 was as distinct from the corona of 1878 as anything could be. In 1871 we got nothing but bright lines indicating the presence of gases, namely hydrogen and another since provisionally called coronium. In 1878 we got no bright lines at all; so I then stated that probably the changes in the chemistry and appearance of the corona would be found to be dependent upon the sun-spot period, and recent work has borne out that suggestion.

I have now specially to refer to the corona as observed and photographed in 1898 in India by means of the prismatic camera, remarking that an important point in the use of the prismatic camera is that it enables us to separate the spectrum of the corona from that of the prominences.

One of the chief results obtained is the determination of the position of several lines of probably more than one new gas, which, so far, have not been recognised as existing on the earth.

Like the lowest hottest layer, *for some reason or other*, this upper layer does not write its record among the lines which build up the general spectrum.

Up to the employment of the prismatic camera insufficient attention had been directed to the fact that in observations made by an ordinary spectroscope no true measure of the height to which the vapours or gases extended above the sun could be obtained; early observations, in fact, showed the existence of glare between the observer and the dark moon; hence it must exist between us and the sun's surroundings.

The prismatic camera gets rid of the effects of this glare, and its results indicate that the effective absorbing layer—that namely, which gives rise to the Fraunhofer lines—is much more restricted in thickness than was to be gathered from the early observations.

We learn from the sun, then, that the absorption which defines its ordinary spectrum is the absorption of a middle region, one shielded both from the highest temperature of the lowest reaches of the atmosphere where most tremendous changes are continually going on, and from the external region where the temperature must be low, and where the metallic vapours must condense.

This is the first great teaching of the test-spectrum. The next chapter will deal with the second.

CHAP. V.—STELLAR ATMOSPHERES.

AFTER the laboratory work undertaken with the view of attempting to find explanations of the various phenomena presented by the sun had reached a certain stage, it became necessary to endeavour to get an idea of the sun's place among the stars by a discussion of all the existing spectroscopic observations which might throw light upon the subject.

At that time a very large number of the most important lines, both bright and dark, recorded in stellar spectra were of unknown origin. The inquiry, therefore, in the case of all the hotter stars had to do with the spectral lines as hieroglyphics, not as special chemical representatives.

When I began the inquiry, the prevailing ideas were that the first period of a star's life was one of the highest temperature, and that all the differences observed were due to different stages of cooling having been reached. With regard to the nebulæ, they, it was imagined, formed a different order of created things from the stars.

Passing over the old views, among them one that the nebulæ were holes in something dark, which enabled us to see something bright beyond; and another that they were composed of a fiery fluid, I may say that not long ago they were supposed to be masses of gases only, existing at a very high temperature; and it was also suggested that they, perchance, represented the residua in space left after all the stars had been formed.

The upshot of this inquiry forms the subject matter of two companion volumes,* so I need not dwell upon it in any detail here. But it is necessary that I should state, as briefly as may be, the results to which the discussion of all the then available spectroscopic observations led me.

All the observations were satisfied by the working hypothesis of the evolution of all cosmical bodies from meteorites, the various stages recorded by the spectra being brought about by the various conditions which follow from the hypothesis.

The nebulæ present us with the first stage. They are taken to be sparse swarms of meteorites colliding together, and thus producing their luminosity, which spectroscopically is found to be due to permanent gases, hydrogen and the cleveite gases and carbon compounds driven out of the meteorites as a result of the heat produced by the collisions; and to a less extent to the low temperature lines of some of the chemical metallic elements known to exist in meteorites.

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* *The Meteoritic Hypothesis* and *The Sun's Place in Nature.* Macmillan.

We have then to deal with the colliding particles of the swarm and the permanent gases given off and filling the interspaces. The temperature is relatively low; since gases may glow at a low temperature as well as at a high one, the temperature evidence depends upon the presence of cool metallic lines and the absence of the enhanced ones.

The nebulæ, then, are relatively cool collections of some of the permanent gases and of some cool metallic vapours, and both gases and metals are precisely those I have referred to as writing their records most visibly in stellar atmospheres.

If the nebulæ are thus composed, they are bound to condense to centres, however vast their initial proportions, however irregular the first distribution of the cosmic clouds which compose them. Each meteorite, the motion of which is stopped by collisions, must at once fall to the centre of gravity of the swarm.

Each pair of meteorites in collision puts us in mental possession of what the final stage must be. We begin with a feeble absorption of metallic vapours round each meteorite in collision; the space between the meteorites is filled with the permanent gases driven out further afield, and having no power to condense. Hence dark metallic and bright gas lines. As time goes on, the former must predominate, for the whole swarm of meteorites will then form a gaseous sphere, with a strongly heated centre, the light of which will be absorbed by the exterior vapour.

As condensation goes on, the temperature at the centre of condensation always increasing, all the meteorites of the parent swarm in time are driven into a state of gas. The meteoritic bombardment practically now ceases for lack of material, and the future history of the mass of gas is, speaking generally, that of a cooling body, the violent motions in the atmosphere while condensation was going on now being replaced by a relative calm, producing a quiescent reversing layer the observation of which alone enables us to define the temperature of the star.

The temperature-order of the group of stars with bright lines as well as dark ones in their spectra, has been traced, and typical stars indicating the spectral changes have been as carefully studied as those in which absorption phenomena are visible alone, so that now there are very few breaks in the line connecting the nebulæ with the stars on the verge of extinction.

We find ourselves here in the presence of minute details exhibiting the workings of a law associated distinctly with temperature; and more than this, we are also in the presence of high temperature furnaces, entirely shielded by their vastness from the presence of those distracting phenomena which we are never free from in the most perfect conditions of experiment we can get here.

Thanks to the spectroscope, the old guesses have now been replaced by the result of a general inquiry, in which hundreds of thousands of observations have been used, and for my part I do not think it probable that the scheme of celestial evolution which I have sketched above

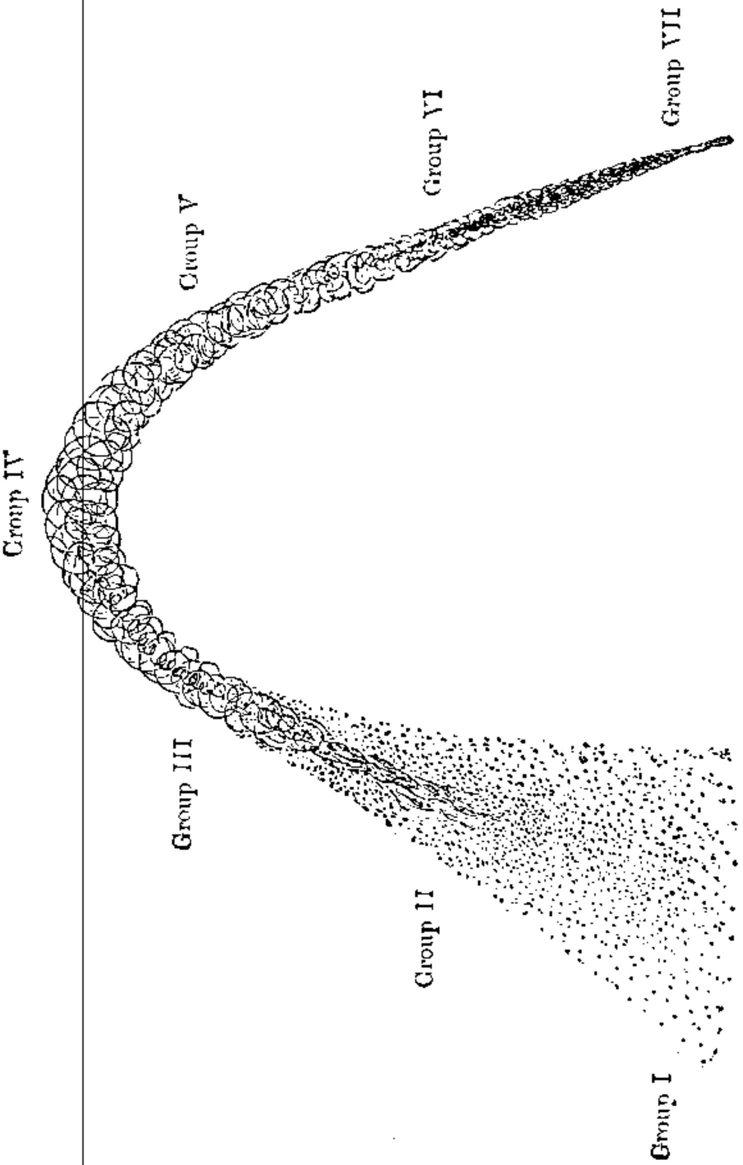

Fig. 24.—Temperature curves.

and which is indicated in the accompanying temperature curve, will be greatly changed in its essential points; it rests upon so wide a basis of induction.

When this view of celestial evolution was first formulated as the result of the wide spectroscopic inquiry to which I have referred, most of the lines in the nebulæ, and in the stellar groups III, IV, and V, were of unknown origin; the groups were established by accepting their presence as criteria, without any reference to chemistry. In the lower groups I, II, and VI, the chemistry was obvious, and the identification of many metallic flutings made it clearer still.

When engaged later on, in 1893, in the classification of stars, according to their *photographic spectra*,* I came across two very important sets of lines of unknown origin, one in the hottest stars, the other in stars of intermediate temperature.

After the discovery of a terrestrial source of helium by Professor Ramsay, I showed in a series of seven notes communicated to the Royal Society,† May—September, 1895, that the cleveite gases, which I obtained by the process of distillation, accounted to a very great extent for the first set.

This result proved to be the key to the chemistry of groups III and IV, which contains the hottest stars.

In 1897, in a series of three communications to the Royal Society,‡ I pointed out that some of the other set of unknown lines in the stars of intermediate temperature, taking *α* Cygni as an example, were due to the enhanced spark lines of iron and other metals, the arc lines being almost entirely absent.

The recent developments of this research, and the ultimate formation of a "test-spectrum," have been referred to in Chapter III. The result of this has been to greatly strengthen the argument based upon the first observations.

In the accompanying photograph, a comparison is shown between the lines of *α* Cygni and the enhanced lines of the substances thrown together to form the "test-spectrum." The extraordinary number of coincidences is seen at a glance. The facts are as follows:—

The number of lines measured in the spectrum of *α* Cygni at Kensington between λ 3798·1 and λ 4861·6 is	307
Of these the number which approximately coincides with the enhanced metallic lines so far observed is	120

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* *Phil. Trans.*, A, vol. clxxxiv, p. 675.

† 1st note, *Proc. Roy. Soc.*, vol. lviii, p. 67; 2nd, *ibid.*, vol. lviii, p. 113; 3rd, *ibid.*, vol. lviii, p. 116; 4th, *ibid.*, vol. lviii, p. 192; 5th, *ibid.*, vol. lviii, p. 193; 6th, *ibid.*, vol. lix, p. 4; 7th, *ibid.*, vol. lix, p. 342.

‡ *Proc. Roy. Soc.*, vol. lx, p. 475; *ibid.*, vol. lxi, p. 148; *ibid.*, vol. lxi, p. 441.

The number of lines (excluding the hydrogen series) in α Cygni of intensity over 4 (the maximum being represented by 10) is ..	40
Of this number, the coincidences with enhanced metallic lines with the dispersion employed amount to	38

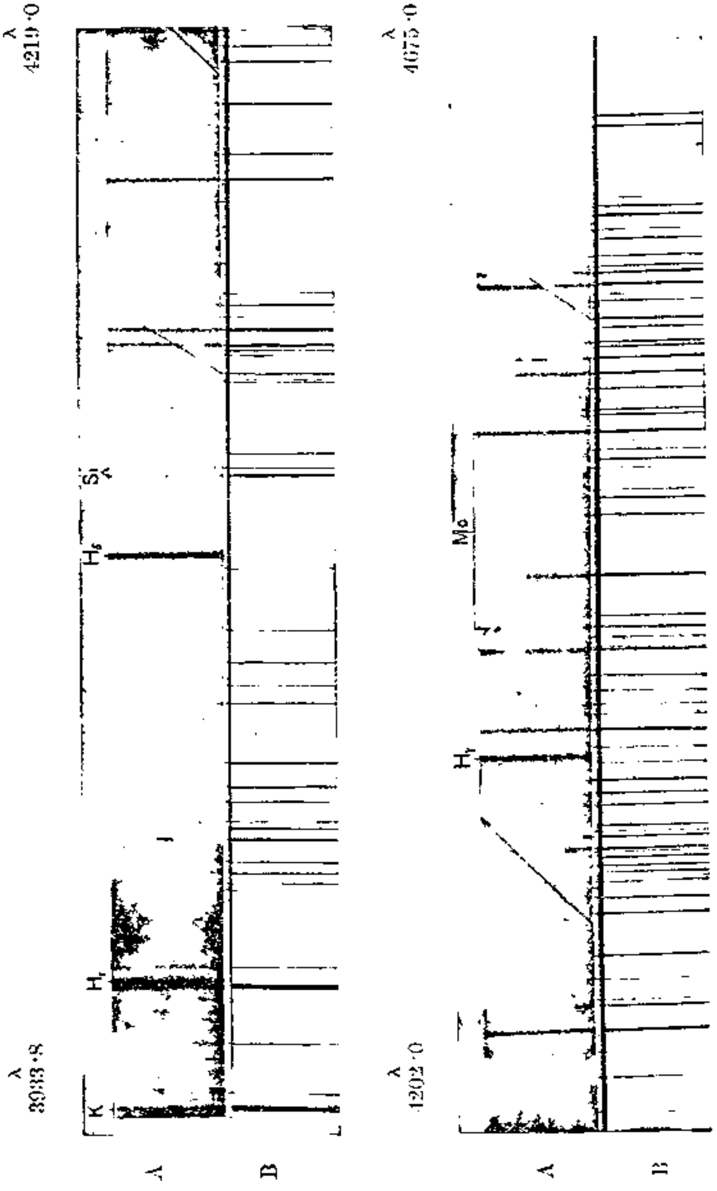

FIG. 25.—Comparison of α Cygni and the enhanced lines of certain metals (chiefly of the iron group).
A spectrum of α Cygni between wave lengths stated.
B test-spectrum of enhanced lines.

The lines of the stars of intermediate temperature, like α Cygni, have long been recognised by the Harvard observers as well as by myself as presenting great difficulties.

In 1893 I wrote as follows:[*] "With the exception of the K line, the lines of hydrogen and the high temperature line of magnesium at λ4481, all the lines may be said to be at present of unknown origin. Some of the lines fall near lines of iron, but the absence of the strongest lines indicates that the close coincidences are probably accidental." In the Harvard *Spectra of Bright Stars*, 1897, p. 5, the following words occur, relating to the same stars: "This system of lines should perhaps be regarded as forming a separate class, as in the case of the Orion lines, and should not be described as 'metallic,' as has just been done in the absence of any more distinctive name."

It will be seen then that the second set of "unknown lines" has now been as effectively disposed of by the determination of the enhanced lines of the metallic elements as the first set was by the discovery of the cleveite gases. The secrets of the "unknown lines" in the hottest stars now stand revealed.

Now that the chemical story is so nearly complete, or at all events so much more complete than it was, we are in a position to inquire what the stars teach us concerning their chemistry; but in the first instance we must examine the origin of the information they afford us, that is, amongst other things, we must study their absorbing conditions, and next their chemistry in relation to temperature.

With regard to the origin of the absorption phenomena, to which, for the most part, our inquiries will be directed; in the case of the sun, we have a star so near us that we can examine the *different parts* of its atmosphere, which we cannot do in the case of the more distant stars.

We have seen in Chapter IV the facts with regard to the sun— that the most valid absorbing layer occupies a certain region in the atmosphere not high up, not at the bottom, but slightly above the bottom—that is the chromospheric—layer.

Now the spectrum of Arcturus resembles the spectrum of the sun almost line for line; what is true for the sun therefore must be equally true for Arcturus, which exactly resembles it. The next point we have to consider is whether the absorption in stars generally, which the spectrum indicates for us, takes place from top to bottom of the atmosphere, or only in certain levels.

In many of these stars the atmosphere may be millions of miles high. In each the chemical substances in the hottest and coldest portions *may* be vastly different; the region, therefore, in which this absorption takes place, which spectroscopically enables us to discriminate star from star, must be accurately known before we can obtain the greatest amount of information from our inquiries.

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[*] *Phil. Trans.*, A, vol. clxxxiv, p. 694.

Assuming that the most valid absorbing vapours in any particular star are all near one temperature, we can proceed to investigate the origins of the spectrum lines by first getting a clue as to the probable temperature from the extent of continuous spectrum, and then inquiring into the presence or absence of the lines which are longest in the spectra of various substances at that temperature. If, however, the absorptions take place at different levels in the atmosphere of a star, the proper spectrum of each substance to be thus investigated can only be determined by a comparison of the stellar with the terrestrial lines of the substance under varying temperature conditions.

This method of looking for the longest lines will fail in the case of stars which are hotter than our hottest spark. In such case, therefore, we must necessarily rely on a comparison with lines which, from our study of the spectra at different temperatures, would most probably be longest in the spectrum at a temperature higher than any at which experiments can be carried on.

It is in connection with such an inquiry as this that the study of the conditions of the sun's atmosphere is of supreme importance, that is why I have devoted the previous chapter to it. It is obvious that a knowledge of the solar conditions must be of the utmost value in enabling us to apply a well-established series of facts, gathered in the case of the star nearest to us, to the phenomena presented by the more distant bodies.

By doing this we have obtained facts which suggest in what parts of the atmosphere the absorption takes place which produces the various phenomena on which the chemical classification can be based; these facts we are bound to accept in a discussion of the origin of stellar absorption in the absence of evidence to the contrary. And we are justified in extending these general conclusions to all the stars that shine in the heavens. I go further than this, and say that in the presence of such definite results, it is not philosophical to assume that the absorption may take place at the bottom of the atmosphere of one star, or at the top of the atmosphere of another. The *onus probandi* rests upon those who hold such views.

So much then, in brief, for solar teachings in relation to the record of the absorption of the lower parts of stellar atmospheres.

If we are justified in arguing from a star with a photosphere as well developed as that of the sun to one in which it is in all probability much less marked in consequence of a much higher temperature, then we must consider that the absorptions which mark out the various star groups are more conditioned by the temperatures of the absorbing regions merely than by the thickness of the absorbing atmospheres, or by the densities of the various vapours. Another consideration to be borne in mind is that if the atmospheres are in part composed of condensable vapours, and not entirely of gases permanent at all stellar temperatures, condensation must always be going on outside at the region of lowest temperature.

The absorption phenomena in stellar spectra are not identical at the same mean temperature on the ascending and descending sides of the curve, on account of the tremendous difference in the physical conditions.

In a condensing swarm, the centre of which is undergoing meteoritic bombardment from all sides, there cannot be the equivalent of the solar chromosphere; the whole mass is made up of heterogeneous vapours at different temperatures, and moving with different velocities in different regions.

In a condensed swarm, of which we can take the sun as a type, all action produced from without has practically ceased; we get relatively a quiet atmosphere and an orderly assortment of the vapours from top to bottom, disturbed only by the fall of condensed metallic vapours. But still, on the view that the differences in the spectra of the heavenly bodies chiefly represent differences in degree of condensation and temperature, there can be, *au fond*, no great chemical difference between bodies of increasing and bodies of decreasing temperature. Hence we find at equal mean temperatures on opposite sides of the temperature curve, this chemical similarity of the absorbing vapours proved by many points of resemblance in the spectra, especially the identical behaviour of the enhanced metallic and cleveite lines.

Now that the test-spectrum has led us to such a very definite conclusion with regard to α Cygni and other stars resembling it, it is necessary to turn back to Chapter IV, in which the solar atmosphere was discussed. It was pointed out what a marvellous resemblance there was between the test-spectrum and the sun's chromosphere, photographed during the eclipse of 1898. If the spectra of the valid absorbing atmosphere of α Cygni and of the sun's chromosphere resemble the test-spectrum as they do, the atmospheres must resemble each other, both in chemistry and temperature.

Here, then, we have an almost undreamt-of opportunity of noting the close connection between solar and stellar phenomena, not merely in noting the identity of the action of the *absorbing layers* as we do when we find the spectra of the sun, Arcturus and Capella, almost identical, line for line, but in studying the relation of the absorbing layer of one star to the underlying layer in another.

While we find, on the one hand, that the absorbing layer of the sun is similar to those of Arcturus and Capella, we find, on the other, that the spectrum of the sun's chromosphere resembles that of the reversing layer of α Cygni. The "test-spectrum" fits them both.

Now the chromosphere by a well-known physical law must be hotter than anything outside it, but we know that the reversing layer lies outside it, therefore the reversing layer of α Cygni must be hotter than the reversing layer of the sun.

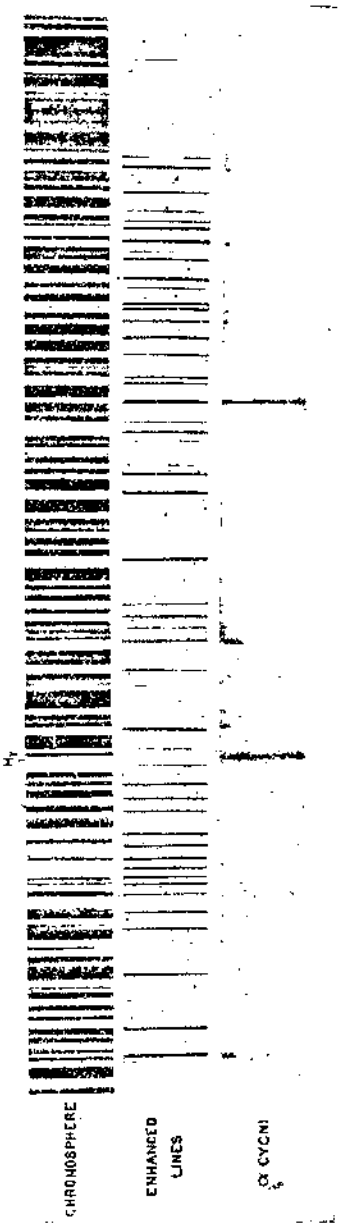

Fig. 26.—Comparison of the spectra of the chromosphere and α Cygni, showing that the test spectrum composed of enhanced lines is common to both.

In the chromosphere of 1898, the enhanced lines are all of greater intensity than the corresponding Fraunhofer lines, and they are also relatively stronger, as referred to the arc lines, than they are in the experimental spark. Hence, the incandescent iron vapour in the chromosphere must be at a temperature at least as high as that of the spark, and certainly higher than that of the iron vapour which is most effective in the production of Fraunhofer lines.

The evidence is complete that the temperature in the reversing layer of α Cygni is higher than that of the reversing layer of the sun. What do we find? Of lines disappearing we have the arc lines of iron, calcium, magnesium, strontium, and so on, some thousands in number. Of lines increasing in importance we have the small number representing the enhanced lines of iron, the lines of hydrogen, and some others which we cannot at present associate with the name of any known substance. Here, then, we get a series of phenomena which is simply and sufficiently explained by the statement that on passing from the temperature of the sun to α Cygni, among other changes brought about, the complicated line spectrum of iron is giving way to a more simple one consisting of the enhanced lines. Further inquiries show that the other metallic spectra are behaving in the same way.

In passing from the absorbing layer of the sun to that of α Cygni, then, we pass from the arc lines of the metallic elements to the enhanced lines. Truly a most tremendous change which the test-spectrum puts beyond all question. The significance of this will come later.

In the case of the sun, the enhanced test-spectrum was the only one we could employ with advantage. But in the case of the hottest stars, stars that is, with the longest spectrum, we can go still further. These are so much hotter than the sun, that they give us the opportunity of noting another break; really of employing another test-spectrum, that afforded by the summation of the lines of hydrogen and the cleveite gases.

As we have seen, the arc metallic lines give way to the enhanced metallic lines in stars of intermediate temperature, like our sun and α Cygni, so, in the hottest stars the enhanced metallic lines vanish almost entirely, and give place to a spectrum almost purely gaseous.

To take iron as an example, for the sake of simplicity, it will be seen then that the actual stellar phenomena might have been predicted up to a certain point, from a consideration of laboratory and solar phenomena. But the stars carry us further than our predictions; we see the gradual increase of hydrogen and the cleveite gases. The facts demonstrate that as temperature increases hydrogen increases, and, together with the cleveite gases not obvious before, finally replaces iron which has disappeared

CHAP. VI.—THE CHEMISTRY OF THE STARS.

THE recent advances in our knowledge which have come from the combination and interaction of solar, stellar and laboratory research, carried on by the aid of instruments of much greater power than those formerly used, have given us a firm chemical hold on all the groups of stars in my classification of them. These groups were established by discussing sequences of lines before the origin of the lines had been made out; as I have already said, a series of hieroglyphics is now replaced by chemical facts; and we can now study the chemistry of the stars, as well as their order in a system of classification.

The first question which naturally arises is this: Do the chemical elements make themselves visible indiscriminately in all the celestial bodies, so that practically, from a chemical point of view, the bodies appear to us of similar chemical constitution? This is not so.

From the spectra of those stars which resemble the sun, in that they consist of an interior nucleus surrounded by an atmosphere which absorbs the light of the nucleus, and which therefore we study by means of this absorption; it is to be gathered that the atmospheres of some stars are chiefly gaseous, *i.e.*, consisting of elements we recognise as gases here, of others chiefly metallic, of others again mainly composed of carbon or compounds of carbon.

Here then we have spectroscopically revealed the fact that there is considerable variation in the chemical constituents which visibly build up the stellar atmospheres.

This, though a general, is still an isolated statement. Can we connect it with another?

By means of one of the first principles of spectrum analysis referred to in Chapter I, we know that the hotter a thing is the light of which produces a continuous spectrum, the further does the spectrum stretch into the violet and ultra-violet.

Hence the hotter a star is, the further does its complete or *continuous* spectrum lengthen out towards the ultra-violet, and, *cæteris paribus*, the less is it absorbed by cooler vapours in its atmosphere.

Now to deal with three of the main groups of stars, we find the following very general result:—

Gaseous stars	Longest spectrum.
Metallic stars	Medium spectrum.
Carbon stars	Shortest spectrum.

We have now associated two different series of phenomena, and we are entitled to make the following general statement:—

Gaseous stars	Highest temperature.
Metallic stars	Medium temperature.
Carbon stars	Lowest temperature.

Hence the differences in apparent chemical constitutions are associated with differences of temperature.

This, then, is the result of our first inquiry into the existence of the various chemical elements in the atmospheres of stars generally. We get a great diversity, and we know that this diversity accompanies changes of temperature. We also find that the sun, which we independently know to be a cooling star, and Arcturus, are identical chemically.

Can we associate with the two to which I have already called attention still a third class of facts?

Laboratory work enables us to do this.

The cleveite gas spectrum and the spectrum of enhanced metallic lines come to our help and enable us to get a step forwarder. In studying the appearance of these lines in stellar spectra, we have a third series of phenomena available, and we find that the results are absolutely in harmony with what has gone before. Thus

Gaseous stars ..	Highest temperature ..	Strong cleveite gas and faint enhanced lines.
Metallic stars ..	Medium temperature ..	Feeble cleveite gas and strong enhanced lines. No cleveite gas, and strong arc lines.
Carbon stars ..	Lowest temperature ..	Faint arc lines.

It is clear now, not only that the spectral changes in stars are associated with, or produced by, changes of temperature, but that the study of the enhanced spark and the arc lines lands us in the possibility of a rigorous stellar thermometry, such lines being more easy to observe than the relative lengths of spectrum.

What then, is the chemical law? It is this. In the very hottest stars we deal, speaking generally, with the gases hydrogen, helium, asterium, and doubtless others still unknown, almost exclusively. At the next lowest temperatures we find these gases being replaced by metals in the state in which they are observed in our laboratories when the most powerful jar-spark is employed. At a lower temperature still the gases almost disappear entirely, and the metals exist in the state produced by the electric arc.

I said "speaking generally," but we really can go further than this general statement, and I next pass from the general to the particular, and give the detailed results recently obtained in the case of stars as hot or hotter than Arcturus—taking Arcturus to represent the solar temperature—in the light of the most recent work, some of which has already been referred to in the preceding chapters.

Proto-metals.

With regard to the metals, the recent work on the enhanced lines in the spectrum of metals, α Cygni* and the sun's chromosphere enables us to deal with the lines observed at the highest temperature in the spectra of the following substances: magnesium, calcium, iron, manganese, nickel, chromium, titanium, copper, vanadium, strontium, silicium.

The untouched reproductions of photographs of the spectra of the chromosphere and α Cygni, given on page 53, have already shown the wonderful similarity which exists between these three spectra.

As we have to deal both with the arc and spark lines of these substances, for the sake of clearness I call the latter "*proto-metallic*" lines, and consider the substances which produce them, obtained at the highest available laboratory temperatures, "proto-metals," that is, a finer form of the metal than that which produces the arc lines, corresponding to the "meta-elements" imagined by Crookes.

The temperature ranges of the enhanced lines of these metals have been investigated in various stars with the following results:—

Metal.	Range of temperature (upward series).	Range of temperature (downward series).
Magnesium	α Ursæ Min. to γ Argûs	α Eridani to Procyon.
Calcium..	α Tauri to γ Argûs	α Eridani to Arcturus.
Iron	α Tauri to ζ Tauri†	β Persei to Arcturus.
Titanium	α Tauri to ζ Tauri	β Persei to Arcturus.
Manganese	α Ursæ Min. to α Cygni	β Persei to Procyon.
Nickel	α Ursæ Min. to α Cygni	β Persei to Procyon.
Chromium	α Ursæ Min. to α Cygni	γ Lyræ to Procyon.
Vanadium	α Ursæ Min. to α Cygni	Sirius to Procyon.
Copper	α Ursæ Min. to α Cygni	β Persei to Procyon.
Strontium	α Tauri to α Cygni	Sirius to Arcturus.

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[*] *Nature*, vol. lxix, p. 342.

[†] This is one of the most extraordinary spectra which has been met with in the Kensington series of photographs, as I have already pointed (*Proc. Roy. Soc.*, vol. lxi, p. 184). While the lines of hydrogen are fairly sharp and not very broad, many of the lines, especially those of the cleveite gases, are broadened almost into

The enhanced lines of the above substances seem to account for almost all of the more marked lines in α Cygni. It is on this ground that I have investigated their behaviour in other stars before waiting for the results of the complete inquiry. Another reason has been that, although in addition to the enhanced lines of the metals shown in the foregoing table, those of barium, cadmium, molybdenum, lanthanum, antimony, lead, palladium, tantalum, erbium and yttrium, tungsten, cerium, uranium, cobalt, and bismuth have already been investigated with lower dispersion, and a spark obtained with the use of a much less jar capacity, so far as I have no certainty that any of these substances exist in the reversing layers of stars of intermediate temperature.

The temperature ranges of the arc lines of some of the metals have also been investigated, and the results are shown in the following table :—

Metal.	Range of temperature (upward series).	Range of temperature (downward series).
Iron	α Tauri to α Cygni.	α Canis Majoris to Arcturus.
Calcium	α Tauri to α Ursæ Min.	α Canis Majoris to Arcturus.
Manganese	α Tauri to α Ursæ Min.	α Canis Majoris to Arcturus.

So much, then, for the metals. I now turn to the gases.

Proto-hydrogen.

Some little time ago Professor Pickering, of the Harvard Observatory, found on examining the spectra of the southern stars, that one of them on the poop (Lat. Puppis), hence called ζ Puppis, of the ship which forms the constellation Argo, contained a system of lines not hitherto recognised, and he naturally concluded that it indicated a new element.* On further inquiry he found reason to suppose that this new series was in some way connected with hydrogen, since the lines occupied the same positions as those computed from the same formula and constants from which the ordinary series of hydrogen was calcu-

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invisibility. On the meteoritic hypothesis this is explained by the great differences of velocity and direction of the meteoritic streams, the special broadening of the lines of the cleveite gases indicating that these gases are chiefly concerned in disturbances at high temperatures.

On account of the indistinctness of many of its lines, ζ Tauri is omitted from the present discussion.

* See *Astrophysical Journal*, vol. iv, p. 369, and vol. 5, p. 95.

lated, the only difference in the employment of the formula being that even values of n were used instead of odd values.*

Professors Pickering and Kayser both concede that this new form of hydrogen is due most probably to a high temperature, and Professor Kayser expressly states "that this series has never been observed before can perhaps be explained by insufficient temperature in our Geissler tubes and most of the stars."

If, as suggested both by Professor Kayser and myself, this new series and the one previously known are probably of the subordinate type, the principal series of hydrogen is still beyond our ken, unless indeed one of the still "unknown" lines represents it, as suggested by Professor Rydberg. Another possibility is that, even in the hottest stars so far considered, the temperature is not high enough to allow its molecule to exist uncombined.

On the view that the new series of probable hydrogen lines in ζ Puppis represents the effect of a transcendental temperature, an attempt has been made to produce this spectrum in the laboratory. In the high-tension spark in hydrogen at atmospheric pressure the ordinary series is represented by broad lines. The use of the spark with large jars in vacuum tubes results in the partial fusion of the glass and the appearance of lines which have been traced to silicium, but the new series has not yet been observed.

In his first communication Professor Pickering mentions lines at 4698, 4652, 4620, and 4505, but he does not refer to them in his second paper, which has special reference to the new series. The line 4505 was at first taken to be one of the components of the new series, but this seems to have been subsequently superseded by the employment of the line about 4544, which agrees better both as regards intensity and the calculated position 4543·6.

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* The two series are as follows:—

Old Series.			*New Series.*		
n.	Computed.	Observed.	n.	Computed.	Observed (means).
6	6563·0	6563·0	5	10128·1	—
8	4861·5	4861·5	7	5413·9	—
10	4340·6	4340·7	9	4543·6	—
12	4101·9	4101·8	11	4201·7	4200·4
14	3970·2	3970·2	13	4027·4	4026·8
16	3889·2	3889·1	15	3925·2	3924·7
18	3835·5	3835·5	17	3859·8	3858·7
20	3798·0	3798·1	19	3815·2	3815·9
			21	3783·4	3783·4

These figures are taken from Professor Pickering's article in *Astrophysical Journal*, vol. v. p. 93. See also Kayser's article on p. 95 of the same [illegible].

As this new hydrogen series seems to bear the same relation to the well-known one as the proto-metallic lines bear to the metallic, I call the gas which produces it proto-hydrogen for the sake of clearness.

The new series of lines has been found in the spectra of ζ, ε, δ, and κ Orionis photographed at Kensington in 1892.

Professor Pickering himself has since found this system of lines in other stars than ζ Puppis, 29 Canis Majoris among them, and Mr. McClean, in his admirable work on the brightest stars of the southern hemisphere, has obtained photographs of the spectrum of γ Argûs, in which the new series appears.

From a discussion of these stars in relation to the others photographed, there can be little doubt that we are here face to face with the very hottest stars so far known: and that the new series of hydrogen lines represents one among the last stages of chemical simplification so far within our ken.

We are, therefore, now in a better position to determine the relation of this new gas to other gases, both known and unknown, appearing in stars of nearly equal temperature.

Other New Lines.

But even with our present knowledge of stellar spectra we find that in relation to the hottest stars there are still some gaps in our chemical knowledge; not only is this so, but have we any right to assume, taking into account the limitations of our means of observation and of the strict limitation of our observations to the relatively small part of space nearest us, enormous though it is, that we are as yet really in touch with the highest stellar temperatures?

Again, we cannot be certain that the small number of stars as yet studied puts us in presence of the highest stellar temperatures. Those stars which apparently are at the very apex of the temperature curve are involved in unknown lines, and require a special study.

Two typical unknown lines have wave-lengths at 4089·2 and 4649·2,* and besides these three other unknown lines occur in γ Argûs.

As these most probably reveal still undiscovered gases, I include them in the following table showing the limits of stellar temperature to which the various known and unknown lines, probably of gaseous origin, extend.

Mr. McClean has stated that certain of the oxygen lines (amongst which is the strong triplet at λλ 4070·1, 4072·4, and 4076·3) appear in the spectrum of β Crucis and other stars of nearly equal temperature. My own observations, so far as they have gone, tend to confirm this

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* *Proc. Roy. Soc.*, vol. lxii, p. 52.

Origin.	λ of chief lines.	Range in ascending series of stars.	Range in descending series of stars.
Unknown ..	4457 4451 3876	Seen only in γ Argûs.	
Hydrogen (new)	4544·0 4200·4	ζ Orionis to γ Argûs	No stars available.
Unknown ..	4649·2	α Crucis to ζ Orionis.	α Eridani.
Helium ..	4471·6 4026·3	Rigel to γ Argûs	α Eridani to γ Lyræ.
Asterium ..	4388 4009	Rigel to γ Argûs	α Eridani to γ Lyræ.
Hydrogen ..	Complete series	Aldebaran to γ Argûs	α Eridani to Arcturus.

view; but other photographs and more laboratory work are needed to explain certain changes of intensity which have been observed. The lines attributed by Mr. McClean to oxygen have been noted between α Crucis and ζ Orionis in the upward series, and in stars at about the α Eridani stage of temperature in the downward series.

There is evidence that the strongest lines of nitrogen at λ 3995·2 and λ 4630·9 make their appearance in stars at about the temperature of α Crucis. These lines appear from Rigel to ζ Orionis in the upward series, and are present in the stars at the α Eridani stage in the downward.

I pointed out many years ago* that at high temperatures the flutings of carbon in the violet are replaced by a line at λ 4267·5. There is a line at this wave-length in the spectra of stars ranging in temperature from that of Rigel to ζ Orionis on the up side, and from α Eridani to β Persei on the downside of the temperature curve.

There is no known line of gases or metals to which this line can be assigned. It is probable, therefore, that carbon exists in stars of the same temperature as that at which oxygen and nitrogen have been traced.

Two lines in the spectrum of silicium (λ 4128·5 and λ 4131·5) have been traced in stars between the temperatures of α Ursæ Min. and α Crucis in the upward series, and between those of α Eridani and Procyon on the downward.

The accompanying map shows the facts relating to stars as hot as, or hotter than, the sun, as we know them at present.

Description of Map.

The map is arranged on the following plan. The temperature of the sun and Arcturus forms the lowest stage. The upper limit is

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* *Proc. Roy. Soc.*, vol. xxx. p. 461.

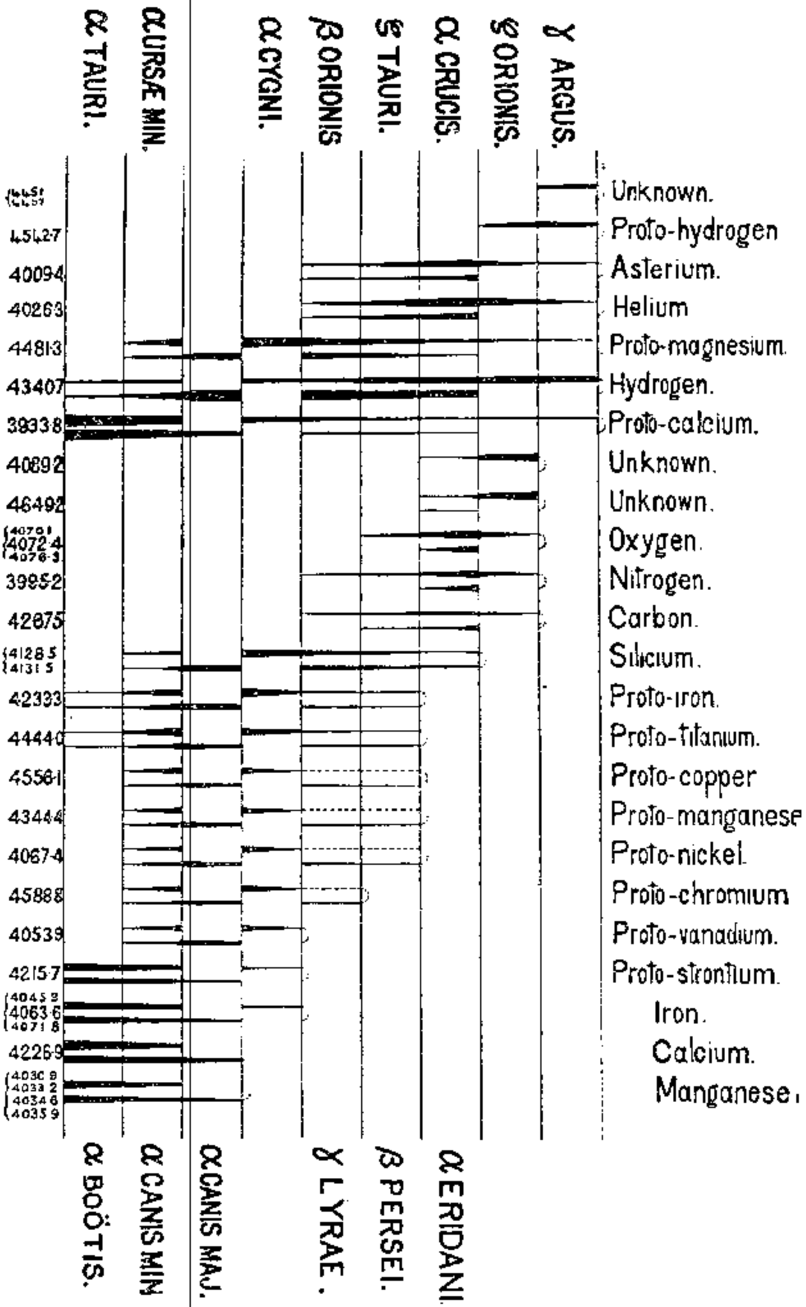

FIG. 27.—Map of chemical substances present in stars of different temperatures.

defined by γ Argus, the hottest star so far known. On the left the stars named are those of increasing temperature, on the right those of decreasing temperature. Those on the same horizon represent equal mean temperatures so far as the cleveite gas and enhanced lines help us to determine them. The blank spaces indicate that so far no star has been photographed in the spectrum of which the enhanced lines exactly match those on the opposite side.

The names of the various chemical substances included in the discussion are given at the top. I have retained the prefix "proto" to that condition of each metallic vapour which gives us the enhanced lines alone, and I have added it to that form of hydrogen seen only in the hottest stars.

The behaviour of the most typical line of each chemical substance is indicated by a double line looped at the top at its highest range. The length and varying thickness of the lines in stars on both sides of the temperature curve are derived from the observed appearance and intensity of the lines, noted in the different stars.

The wave-lengths of the lines discussed are shown at the bottom of the map.

Details of Changes observed.

The facts embodied in the map present to us the spectral changes noted in stars of Groups III, IV, and V of my classification,* and are a result of a more general inquiry than those referred to in my previous papers † the origins of a very considerable number of stellar lines having since then been traced to enhanced lines of metals and to known gases.

It will be seen that this more general inquiry entirely justifies the prior statement‡ that the metallic lines are thickest in stars increasing their temperature, and the hydrogen lines thickest in stars decreasing their temperature, in other words, on the opposite arms of the temperature curve. I have already stated a possible explanation.§

It will be observed that, so far, I have not been able to find stellar spectra on the downward side corresponding to those of γ Argûs and ζ Orionis; but it is more than probable that near the apex of the curve only a small change will be observed; their default, therefore, is of less consequence than it might have been.

The same remark applies to α Cygni and Sirius: but here it is certain that the differences in the relative intensities of the gaseous and enhanced lines will be considerable, judging from what happens above and below the heat stages represented by them.

The stars used in the discussion give us very definite results, showing that the various chemical forms are introduced at six very distinct heat levels.

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* *Proc. Roy. Soc.*, vol. xliii, p. 117 (1887).

† *Proc. Roy. Soc.*, vol. xliv, p. 1 (1888); *ibid.*, vol. xlv, p. 380 (1889); *Phil. Trans.*, A., 184, (1893), p. 725.

‡ *Proc. Roy. Soc.*, vol. lxi, p. 182.

§ *Proc. Roy. Soc.*, vol. lxi, p. 183.

The Temperature Ranges.

I next proceed to make some remarks upon the series of facts now for the first time brought together; it must, however, be borne in mind that all the chemical elements and all parts of the spectrum have not yet been included in the survey.

The facts indicate individual peculiarities; some chemical forms appear to be longer lived than others, and, further, the important spectral changes in the case of different substances do not occur at the same temperature.

(1) Hydrogen appears throughout both series of stars from top to bottom. Proto-magnesium and proto-calcium follow suit very nearly; but the highest intensity of the former is reached at the stage represented by α Cygni, and of the latter at the solar temperature represented by α Tauri and Arcturus.

(2) With the above exceptions all the chemical forms so far traced are relatively short-lived.

This is the first important differentiation. In the light of (1) we are justified in assuming that the substances in (2) would be visible in the stellar reversing layers if they were there.

(3) In the stars of higher temperatures we deal generally with gases. Below the stages represented by β Orionis and γ Lyræ we deal with proto-metals and metals, hydrogen being the only exception.

(4) The proto-metals make their appearance at about the same heat-level at which the gases (with carbon), always excepting hydrogen, begin to die out.

This is the second important differentiation. It is interesting to notice the distinct difference of behaviour of carbon and silicium in the descending series; the former goes through the same stages as oxygen and nitrogen, the latter behaves like the proto-metals.

(5) With the exception of iron the metals, as contra-distinguished from the proto-metals, only make their appearance in stars at and below the heat level of Sirius.

This is the third important differentiation. It is accompanied with a notable *diminution* of hydrogen and proto-magnesium, and with an *increase* of proto-calcium; indeed, the latter seems generally to vary inversely with the hydrogen.

The question arises whether the order of visibility at reduced temperatures now indicated does not explain the absence of proto-hydrogen, oxygen, and nitrogen from the spectra of the sun and nebulæ; the metals present in, and the absence of quartz from, meteorites, and the similarity of the gaseous products obtained from meteorites and metals, native and other, in vacuo at high temperatures.

The Chemistry of the Cooler Stars.

I have shown, on page 57, how the discovery of new lines in the spectra of the metallic elements by using the most powerful induction coil in existence has put us in possession of the chemistry of stars of intermediate temperature; and, further, how the discovery of the cleveite gases has helped us in tracing the origins of very many lines of the hotter stars.

Our knowledge of the chemistry of the cooler stars is little short of marvellous; we have two distinct groups of coolest ones, the evidence of their much lower temperature being the shortness of their spectra. In one of these groups we deal with absorption alone, as in those already considered; we find an important break in the phenomena observed; helium, hydrogen, and the enhanced lines of metals have practically disappeared, and we deal with metallic arc lines and carbon absorption chiefly.

But the other group of coolest stars presents us with quite new phenomena. We no longer deal with absorption alone, but accompanying it we have radiation, so that the spectra contain both dark lines and flutings and bright ones. Now such spectra are visible in the case of *new stars*, as they are called, the ephemera of the skies, which may be said to exist only for an instant relatively. In the case of these bodies, when the disturbance which gives rise to their sudden appearance has ceased, we find their places occupied by nebulæ; we cannot, therefore, be dealing here with stars like the sun, which has already taken some millions of years to slowly cool, and requires more millions to complete the process into invisibility.

Hence in this class of coolest "stars" we are obviously dealing with swarms of meteorites, the condensation of which has scarcely commenced, and hence it is that this class provides us with more "variable stars" than any other.

CHAPTER VII.—A CHEMICAL CLASSIFICATION OF STARS.

IN the attempts made to classify the stars by means of their spectra, from Rutherfurd's time to quite recently, the various criteria selected were necessarily for the most part of unknown origin; with the exception of hydrogen, calcium, iron and carbon, in the main, chemical origins could not be assigned with certainty to the spectral lines. Hence the various groups defined by the behaviour of unknown lines were referred to by numbers, and as the views of those employed in the work of classifying differed widely as to the sequence of the phenomena observed, the numerical sequences vary very considerably, so that any co-ordination becomes difficult and confusing.

The recent work referred to in the last chapter has thrown such a flood of light on the chemistry of the stars that most definite chemical groupings can now be established, and the object of the present chapter is to give an account of the general scheme of classification in which they are employed, which I have recently proposed.

The fact that most of the important lines in the photographic region of the stellar spectra have now been traced to their origins renders this step desirable, although many of the chemical elements still remain to be completely investigated from the stellar point of view.

The scheme is based upon a minute inquiry into the varying intensities, in the different stars, of the lines and flutings of the undermentioned substances:—

Certain unknown elements (probably gaseous, unless their lines represent "principal series") in the hottest stars, and the new form of hydrogen discovered by Professor Pickering (which I term "protohydrogen" for the sake of clearness), hydrogen, helium, asterium, calcium, magnesium, oxygen, nitrogen, carbon, silicium, iron, titanium, copper, manganese, nickel, chromium, vanadium, strontium; the spectra being observed at the highest available spark temperatures. The lines thus observed I term "enhanced" lines, and I distinguished the kind of vapour which produces them by the affix "proto," *e.g.*, protomagnesium, for the sake of clearness.*

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* *Roy. Soc. Proc.*, vol. lxiv, p. 398.

Iron, calcium, and manganese at arc temperatures.

Carbon (flutings) at arc temperatures.

Manganese and iron (flutings) at a still lower temperature.

In the last chapter I stated the results arrived at recently with regard to the appearances of the lines of the above substances in stars of different temperatures, and the definitions of the different groups or genera to be subsequently given are based upon the map given on page 62, together with more minute inquiries on certain additional points the examination into which was suggested as the work went on.

So far as the inquiry has at present gone, the various most salient differences to be taken advantage of for grouping purposes are represented in the following stars, the information being derived from the researches of Professor Pickering* and Mr. McClean,† as well as from the Kensington series of photographs.

Hottest Stars.

Two stars in the constellation Argo (ζ Puppis and γ Argûs ‡).

Alnitam (ε Orionis). This is a star in the belt of Orion shown on maps as Alnilam. Dr. Budge has been good enough to make inquiries for me, which show the change of word to have been brought about by a transcriber's error, and that the meaning of the Arabic word is "a belt of spheres or pearls."

Stars of intermediate Temperature.

Ascending Series.	*Descending Series.*
β Crucis.	Achernar.
ζ Tauri.	Algol.
Rigel.	Markab.
α Cygni.	[ ]
[ ]	Sirius.
Polaris.	Procyon.
Aldebaran.	Arcturus.

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* *Astro-phys. Journ.*, vol. v, p. 92, 1897.

† *Spectra of Southern Stars.*

‡ The spectrum of this star contains bright lines, but when these occur with dark lines, the latter alone have to be considered for purposes of chemical classification.

Stars of lowest Temperature.

Ascending Series.	*Descending Series.*
Antares, one of the brightest stars in Dunér's Catalogue of Class III*a*.[*]	19 Piscium, one of the brightest stars in Dunér's Catalogue of Class III*b*.
[Nebulæ.]	[Dark Stars.]

In order to make quite clear that both an ascending and a descending series must be taken into account, I give herewith two photographs showing the phenomena observed on both sides of the temperature curve in reversing layers of stars of nearly equal mean temperatures, as determined by the enhanced lines. The stars in question are:—

Sirius (descending). α Cygni (ascending).	Fig. 28.
Procyon (descending). γ Cygni (ascending).	Fig. 29.

The main differences to which I wish to draw attention are the very different intensities of the hydrogen lines in Sirius and α Cygni, and the difference in the width and intensities of the proto-metallic and metallic lines in Procyon and γ Cygni. These differences, so significant from a classification point of view, were first indicated in a communication to the Royal Society in 1887[†], and the progress of the work on these lines has shown how important they are. I have based the group—or generic—words upon the following considerations.

As we now know beyond all question that a series of geological strata from the most ancient to the most recent brings us in presence of different organic forms, of which the most recent are the most complex; is it possible that the many sharp changes of spectra observed in a series of stars from the highest temperature to the lowest, bring us in presence of a series of chemical forms which become more complex as the temperature is reduced? If so, we are in the stars studying the actual facts relating to the workings of inorganic evolution on parallel lines to those which have already been made available in the case of organic evolution. I shall discuss this matter later.

In the meantime, regarding the typical stars as the equivalents of the typical strata, such as the Cambrian, Silurian, &c., it is convenient

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[*] *Sur les étoiles à spectres de la troisième classe.*

[†] *Proc. Roy. Soc.*, vol. xliii, p. 145.

FIG. 28.

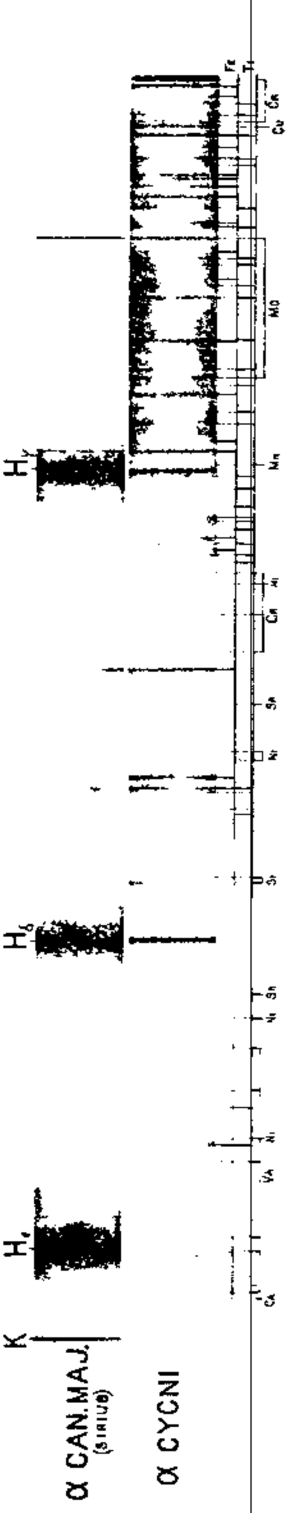
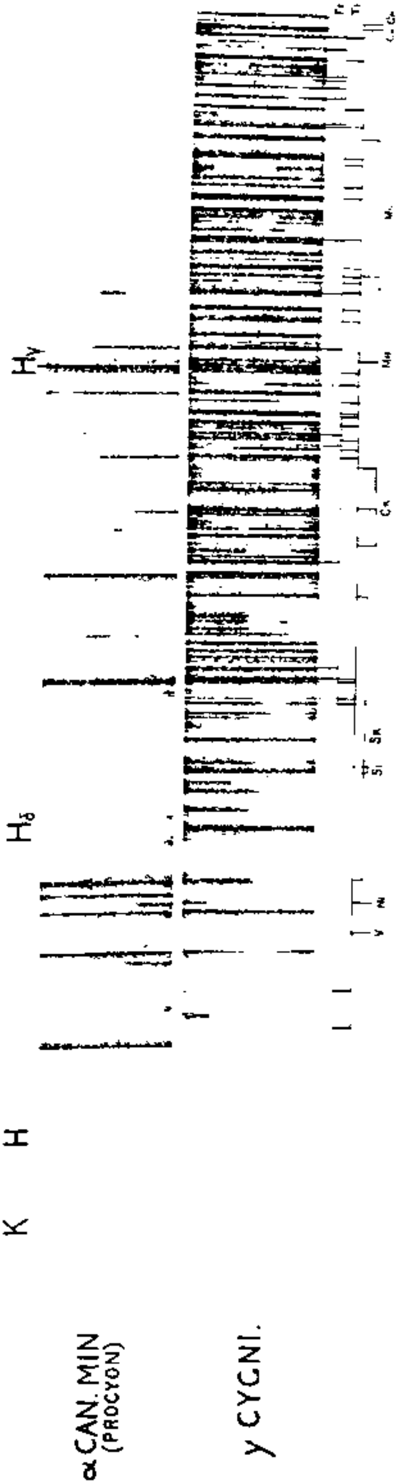

α CAN. MAJ. (SIRIUS)
α CYGNI
K
Hε
Hδ
Hγ

FIG. 29.


α CAN. MIN (PROCYON)
γ CYGNI
K
H
Hδ
Hγ

that the form of the words used to define them should be common to both; hence I suggest an adjectival form ending in *ian*. If the typical star is the brightest in a constellation, I use its Arabic name as root; if the typical star is not the brightest, I use the name of the constellation.

The desideratum referred has to a certain extent determined the choice of stars where many were available. I have to express my great obligations to Dr. Murray for help generously afforded in the consideration of some of the questions thus raised. The table runs as follows:—

CLASSIFICATION OF STARS INTO GENERA DEPENDING UPON THEIR CHEMISTRY AND TEMPERATURE.

Highest temperature, simplest chemistry.

Argonian.
Alnitamian.

Ascending Series.	*Descending Series.*
Crucian.	Achernian.
Taurian.	Algolian.
Rigelian.	Markabian.
Cygnian.	——
——	Sirian.
Polarian.	Procyonian.
Aldebarian.	Arcturian.
Antarian.	Piscian.

The chemical definitions of the various groups or genera are as follows:—

DEFINITIONS OF STELLAR GENERA.

Argonian.

Predominant.—Hydrogen and proto-hydrogen.

Fainter.—Helium, unknown (λ 4451, 4457), proto-magnesium, proto-calcium, asterium.

Alnitamian.

Predominant.—Hydrogen, helium, proto-silicium, unknown (λ 4649·2).

Fainter.—Asterium, proto-hydrogen, proto-magnesium, proto-calcium, oxygen, nitrogen, carbon.

Crucian.	*Achernian.*
Predominant.—Hydrogen, helium, asterium, oxygen, nitrogen, carbon. *Fainter.* — Proto-magnesium, proto-calcium, proto-silicium, unknown (λ 4649·2), silicium.	Same as Crucian.

Proto-metallic lines relatively thick, hydrogen relatively thin.	Proto-metallic lines relatively thin, hydrogen relatively thick.
Taurian. *Predominant.*—Hydrogen, helium, proto-magnesium. asterium. *Fainter.*—Proto-calcium, silicium, nitrogen, carbon, oxygen, proto-iron, proto-titanium.	*Algolian.* *Predominant.*—Hydrogen, proto-magnesium, proto-calcium, helium, silicium. *Fainter.*—Proto-iron, asterium, carbon, proto-titanium, proto-copper, proto-manganese, proto-nickel.
Rigelian. *Predominant.*—Hydrogen, proto-calcium, proto-magnesium, helium, silicium. *Fainter.*—Asterium, proto-iron, nitrogen, carbon, proto-titanium.	*Markabian.* *Predominant.*—Hydrogen, proto-calcium, proto-magnesium, silicium. *Fainter.*—Proto-iron, helium, asterium, proto-titanium, proto-copper, proto-manganese, proto-nickel, proto-chromium.
Cygnian. *Predominant.*—Hydrogen, proto-calcium, proto magnesium, proto-iron, silicium, proto-titanium, proto-copper, proto-chromium. *Fainter.*—Proto-nickel, proto-vanadium, proto-manganese, proto-strontium, iron (arc).	
	Sirian. *Predominant.*—Hydrogen, proto-calcium, proto-magnesium, proto-iron, silicium. *Fainter.*—The lines of the other proto-metals and the arc lines of iron, calcium, and manganese.
Polarian. *Predominant.* — Proto-calcium, proto-titanium, hydrogen, proto-magnesium, proto-iron, and arc lines of calcium, iron, and manganese. *Fainter.*—The other proto-metals and metals occurring in the Sirian genus.	*Procyonian.* Same as Polarian.
Aldebarian. *Predominant.*—Proto-calcium, arc lines of iron, calcium, and manganese, proto-strontium, hydrogen. *Fainter.*—Proto iron and proto-titanium.	*Arcturian.* Same as Aldebarian.
Antarian. *Predominant.*—Flutings of manganese. *Fainter.*—Arc lines of metallic elements.	*Piscian.* *Predominant.*—Flutings of carbon. *Fainter.*—Arc lines of metallic elements.

We may take for granted that as time goes on new intermediate genera will have to be established; the proposed classification lends itself conveniently to this, as there are no numerical relations to be disturbed.

A still more general chemical classification is the following, it being understood that in it only the most predominant chemical features are considered, and that there is no sharp line of separation between these larger groups. The peculiar position of calcium and magnesium renders this caveat the more necessary.

CLASSIFICATION OF STARS.

Highest temperature.

Gaseous stars	Proto-hydrogen stars	Argonian.	
		Alnitamian.	
	Cleveite-gas stars	Crucian.	Achernian.
		Taurian.	Algolian.
Proto-metallic stars		Rigelian.	Markabian.
		Cygnian.	——
		——	Sirian.
Metallic stars		Polarian.	Procyonian.
		Aldebarian.	Arcturian.
Stars with fluted spectra		Antarian.	Piscian.

Lowest temperature.

The detailed chemical facts to be gathered from the definitions of the several genera indicate many important differences between the order of appearance of the chemical substances in the atmospheres of the stars and that suggested by the hypothetical "periodic law." I shall refer to this point later on.

BOOK III.—THE DISSOCIATION HYPOTHESIS.

CHAPTER VIII.—RECENT OPINION.

WHEN stating in Chapter II some of the difficulties encountered by the early workers in spectrum analysis who found it impossible to reconcile the facts which the new method of work was accumulating with the then received chemical view, I pointed out that as early as 1873 I had suggested that many of our difficulties would vanish if it were conceded that the "atoms" of the chemist were broken up, or dissociated, into finer forms by the high temperatures necessarily employed in the new method of investigation.

The year 1873 was 27 years ago; I propose, therefore, to briefly refer, as judicially as I can, to the recent state of opinion on this subject, or rather on some of the main points of it.

Only some of the views I had brought forward from time to time have received general acceptance, those include the breaking up of the solid metal giving (from whatever cause) a continuous spectrum into smaller molecular groupings giving fluted and line spectra.

My view as to the subsequent dissociation of molecules, when once the line spectrum stage has been reached, was still rejected by many. For myself, I am not surprised at this. In a question of such transcendental importance, caution must be redoubled; an absence of work and expression of opinion in such a line of inquiry with questions of pure science only involved, is almost inherent to the nature of the investigations. The chemist has little interest in an appeal to celestial phenomena, and astronomers do not generally concern themselves with chemistry. The region investigated by the chemist is a low temperature region dominated by monatomic and polyatomic molecules. The region I have chiefly investigated is a high temperature region, in which mercury gives us the same phenomena as manganese. In short, the changes with which spectrum analysis has to do take place at a far higher temperature level than that employed in ordinary chemical work, and hence probably it is that I can only refer to one chemical experiment bearing on the subject.

It is important, however, to point out that in cases where the two regions overlap, vapour density determinations and other work have been in harmony with the spectroscopic results, *e.g.*, the changed density of iodine at changed temperatures and with a change in spectrum.

The specific gravity of iodine vapour was found by Deville and Troost to be 8·72 (air = 1), which corresponds to the density 125·9, proving that the molecule or two volumes of iodine gas weighs 126·53 × 2 = 253·06. When iodine vapour is heated to 700° its specific gravity begins to diminish until at higher temperatures it becomes constant, and is half that at 700°, the vapour consisting of free atoms.[*]

Another, but less direct, argument in favour of dissociation, independently of the changes in the intensities of the lines, was based upon some observations I had made in an attempt to work out a spectroscopic method for the detection of impurities. I noted the presence of what I termed "basic lines," that is, short lines which remained common to two or more spectra, after "long lines" had been eliminated as being due to impurities.

I now refer to these different points *seriatim*.

Flutings represent Vibrations of Complex Molecules.

I take the change of the continuous spectrum successively into flutings and lines first, and in justification of the statement that in this matter my view is now generally accepted, I give the following quotations from Schuster and Eder and Valenta :—

"That the discontinuous spectra of different orders (line and band spectra) are due to different molecular combination I consider to be pretty well established, and analogy has led me (and Mr. Lockyer before me) to explain the continuous spectra by the same cause; for the change of the continuous spectrum to the line or band spectrum takes place in exactly the same way as the change of spectra of different orders into each other."[†]

"Später führte Lockyer weiter aus, dass die Gase, solange ihre Moleküle aus mehreren Atomen zerfallen, Linienspectren geben müssen. Diese Anschauung wurde seither ziemlich allgemein acceptirt."[‡]

The question of flutings was early conceded generally, but special exceptions were made, carbon furnishes one instance.

Messrs. Liveing and Dewar in 1879[§] objected to my hypothesis, that the sets of carbon flutings in the green represent molecular groupings of that substance other than that (or those) which gives us the line spectrum, as gratuitous. I showed that the flutings, which Messrs. Liveing and Dewar ascribed to a hydrocarbon, were present in the spectrum of tetrachloride of carbon which gave no trace of hydrogen. This experiment at first gave them no reason to modify their conclusion, but later they repeated and endorsed it, and finally admitted that "the spectrum of the flame of hydrocarbons is not necessarily connected with the presence of hydrogen,"* and so far as I can understand their paper they seem to accept the idea of different molecular groupings, which they began by characterising as "gratuitous."

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[*] Vict. Meyer, *Ber. Deutsch. Chem. Ges.*, vol. xiii, pp. 394, 1010, 1103; Meier and Crafts, *Compt. Rend.*, vol. xc, p. 690; vol. xcii, p. 39.

[†] Schuster, *Phil. Trans.*, 1879, Part I, vol. clxix, p. 39.

[‡] Eder and Valenta, *Denkschriften der kaiserlichen Akademie der Wissenschaften*, Wien, vol. lxi, p. 426, 1894.

[§] *Proc. Roy. Soc.*, vol. xxx, p. 598.

The Complexity of the Line Spectrum.

With regard to the view that the line spectrum integrates for us the vibrations of several sets of molecules, as I have already stated this was not accepted. The number of objections is legion, and it is impossible to refer to all of them here. But, at the same time, the opinion of some of those workers who have approached the subject from both points of view was, I think, coming round to my side, and I shall briefly refer to one or two instances.

Attention has recently been drawn to the variations in the appearance of the magnesium lines in the celestial bodies by Dr. Scheiner, of the Potsdam Observatory, who is not apparently acquainted with my work of 1879; he, however, accepts the idea that the variations furnish us with a precise indication of stellar temperature,† and he is now employing it in the work of the observatory.‡

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* *Proc. Roy. Soc.*, vol. xxxiv, p. 423.

† *Astronomical Spectroscope,* Frost's Translation, p. viii.

‡ Dr. Scheiner points out that in the spectra of nearly all stars of Class Ia (Group IV) the line at 4481 "generally appears as a broad line—in some spectra as strong as the hydrogen lines—but its intensity decreases just in proportion as the number of lines in the stellar spectrum increases, so that it is hardly of the average intensity in the solar spectrum, or other spectra of type IIa, and the author is unable to detect it in the spectrum of α Orionis." My prior work, dating from 1879, being probably unknown to Dr. Scheiner, Messrs. Liveing and Dewar are credited with the discovery of the peculiar behaviour of this line in laboratory experiments, and it is added that "the dependence of the line upon the temperature thus readily suggests that the temperature of the absorbing vapours upon the stars of Class IIIa (Group II) is something like that of the electric arc, while that of the stars of Class IIa is higher, and that of stars of Class Ia is at least as high as the temperature of the high-tension spark from a Leyden jar. This view receives striking confirmation in the precisely opposite behaviour of the magnesium line at λ 4352·18. First becoming visible in the spectra of type Ia (Group IV), which have numerous lines, it is strong in the spectra of type IIa (Groups III and V), and increases so as to be one of the strongest lines as we pass towards type IIIa

Professors Eder and Valenta thus state the conclusions they have recently arrived at in their study of the changes in the spectrum of mercury:—

"Ferner ist die Erscheinung der ziemlich unvermittelten Aufblitzens des linien-reichsten Spectrums (siehe die Abbildung, Fig. 8, der heliographirten Tafel) bei hochgradig gesteigerter Stärke des Flaschenfunkens und gleichzeitigem Erhitzen der Capillare, besonders das Auftauchen zahlreicher neuer Hauptlinien, welche früher nicht oder kaum sichtbar wären, und mancher Doppellinien an Stelle von einfachen Linien, eine derartige, dass sie zu Lockyer's Theorie der Dissociation der Elemente passen würde, wenn man überhaupt die Zerlegbarkeit unserer Elemente in die Discussion ziehen will."*

[Translation:—

"Moreover the appearance of the great brilliancy of the richly lined spectrum with a high tension jar spark, the capillary being heated, and especially the interchange of a great number of new lines which were dim before, and also the change of single lines into double ones; these are such that would harmonise well with Lockyer's theory of dissociation of the elements, if one is prepared to bring into the discussion the possibility of the dissociation of the chemical elements."]

I am glad to be able to quote the following opinion of Sir William Crookes,† to which I attach great weight:—

"Until some fact is shown to be unreconcilable with Mr. Lockyer's views, we consider ourselves perfectly justified in giving them our provisional adhesion, as a working hypothesis to be constantly tested by reference to observed phenomena."

I am anxious to refer here also to the opinion expressed by my colleague, Professor Sir William Roberts-Austen, whose researches have mostly been carried on at high temperatures:—

"Mr. Lockyer has, however, since done far more: he has shown

(Group II). Now, as was found by Liveing and Dewar, this line exhibits just the same peculiarities in the laboratory; in the spark spectrum it is hardly recognisable, in the arc spectrum it is very strong."

My most recent work suggests that Dr. Scheiner is wrong in identifying the magnesium line 4352·18 in the cooler stars with the line nearly in the same position in the hotter stars. In the hot stars the line behaves almost exactly like the enhanced line of magnesium 4481·3, and I have previously pointed out that the stellar line was therefore possibly not due to cool magnesium. This is now justified by the discovery of an important enhanced line of iron at 4351·93, which accounts for the line in the hot stars, and really strengthens Dr. Scheiner's argument.

* *Denkschriften der kaiserlichen Akademie der Wissenschaften,* Wien, vol. lxi, p. 429, 1894.

† *Chem. News,* 1879, vol. xxxix, p. 65.

that the intense heat of the sun carries the process of molecular simplification much farther; and, if we compare the complicated spectra of the vapours of metals produced by the highest temperatures available here with the very simple spectra of the same metals as they exist in the hottest part of the sun's atmosphere, it is difficult to resist the conclusion that the atom of the chemist has itself been changed. My own belief is that these 'atoms' are changed, and that iron, as it exists in the sun, is not the vapour of iron as we know it upon earth."*

The Basic Lines.

With regard to the basic line part of the inquiry, I think I shall not be going too far in saying that it has been universally rejected, and chiefly on the ground that some lines which appeared coincident at the dispersion I employed appeared double with higher dispersions. I have pointed out in the *Chemistry of the Sun* (p. 377) that this is not a sufficient answer, but I have left aside this branch of the inquiry for some years in the hope that some chemist would take up the question of spectroscopic impurities out of which it grew. But it is evident that this basic line point of view, even though it be considered a less direct attack on the problem than others since begun, assumes a much more important and definite position in the light of the new work. I will not go into this question at length now, but will content myself here by asking whether one actual demonstration of dissociation will not take a form very like that which the chemist has taken to be a proof of the existence of impurities.

I shall return to this later on.

Other Physical Researches now in progress.

So much for opinion a year or two ago. In subsequent chapters I shall refer to other attacks upon the problem of dissociation, which to my mind and to many of the objectors sets the matter on a much firmer basis by accumulating facts, not only with regard to the stars, but in other fields of inquiry in which the idea of dissociation has to be appealed to in order to explain the phenomena.

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* *Proc. Roy. Inst.*, vol. xiii, p. 509, 1892.

CHAPTER IX.—THE STELLAR EVIDENCE.

I NEXT proceed to consider whether the views which I found necessary to enable me to group together harmoniously and continuously solar phenomena years ago when nothing was known of stellar chemistry, are weakened or strengthened by the study of the enormous new field of investigation opened out by the recent stellar work, by which we have finally the sun taking its place as one term in a long series, the complete study of which enables us to watch the workings of the celestial evolution which has built up the heavens as we know them.

The great increase of our knowledge we have gained from the study of stars arises from the fact that they have revealed to us a continuous series of spectral changes at temperatures much higher than the sun affords us.

One of the minor advantages of this is, that we can, taking the sun as our base, see what would happen if the sun were to become hotter. Let us consider this point first.

In approaching this part of the subject, it is necessary to proceed with great caution, since the things observed are different. The solar work has consisted in observing different parts of the sun, the star work puts us in presence of the total effects both of radiation and absorption in the case of each body observed.

The facts with regard to the lower portions of the solar atmosphere have already been detailed. They have been gathered from the photographs secured during the eclipse of 1898.

Having these unimpeachable series of facts to go upon, we have found that the absorption indicated by the Fraunhofer lines is not caused by the chromosphere, and that the most valid absorbing layer lies above the chromosphere. We have also seen that in the chromosphere we find enhanced lines among the Fraunhofer lines, which are chiefly arc lines. What must happen then if the sun is supposed to get hotter?

It is only possible to consider the results produced by a higher temperature on two hypotheses. The first, the usual one, that the chemical elements are indestructible; the second, that they are not.

On the first hypothesis it is difficult to say what change could take place which would alter the characteristics of the Fraunhofer spectrum very widely. We have a complex mixture of the vapours of metallic substances and gases with paramount calcium, hydrogen, and the cleveite gases. Temperature cannot therefore vary the relative intensities of the lines. H and K, the chief lines of calcium, must always remain predominant, iron must remain because it cannot be destroyed, and since the quantity of hydrogen and the cleveite gases present cannot be increased, their lines cannot therefore become more important in the spectrum.

It is also clear that any change of relative density on the usual hypothesis cannot be brought about by an increase of temperature; this, then, cannot alter, it cannot change the relative proportions of chemical substances present in any layer, and therefore the relative intensities of the lines which indicate the existence of the various substances in the different layers.

If now we turn to the other hypothesis, that, namely of dissociation, we see at once, in the light of laboratory experiments, that with every considerable increase of temperature in all such masses of vapour and gas as those which now constitute the solar chromosphere and reversing layer, a fundamental change in the appearance of the spectrum must be brought about; complex molecules would be broken up into simpler ones, and the result of this action would bring new lines into the spectrum, indicating the vibration of the molecules produced. Now let us come to facts. Were the temperature of the reversing layer to be increased, if dissociation takes place at this temperature, the dissociation products must become visible, and we must look for them among those lines which expand at the expense of those which contract and disappear. Is any such experiment as this going on even at this moment? The answer is beyond question.

The lower, hotter chromosphere differs from the reversing layer precisely because this change has taken place. As I have said before, we pass on descending the sun's atmosphere from the arc lines in the reversing layer to the enhanced lines in the chromosphere, from the arc spectrum to the "test spectrum," from the metals to the protometals.

What could only be pointed out with regard to only a line or two 20 years ago can now be proved for a whole set of lines, and the dissociation argument is seen to be vastly strengthened the more it is tested.

Next, let us see where the stellar evidence helps us; here I shall deal with the main outlines merely. If in the sun the chromosphere is hotter than the reversing layer in a star slightly hotter than the sun, the reversing layer which builds up the stars' absorption should resemble the chromosphere.

I have already stated the facts with regard to α Cygni. Now let us look at them in the light of the dissociation hypothesis.

The evidence is complete that the temperature in the reversing layer of α Cygni is higher than that of the reversing layer of the sun. What do we find? Of lines disappearing we have are lines of iron, some thousands in number, calcium, magnesium, strontium, and so on. Of lines increasing in importance we have the small number representing the enhanced lines of iron, the lines of hydrogen, and some others which we cannot at present associate with the name of any known substance. Here, then, we get a series of phenomena which, on the hypothesis we are discussing, is simply and sufficiently explained by the statement that on passing from the temperature of the sun to that of α Cygni, among changes brought about the complicated line spectrum of iron is giving way to a more simple one consisting of the enhanced lines. Further inquiries show that the other metallic spectra are behaving in the same way. Looking for the lines which increase in importance, while the others are reduced, we find the lines of hydrogen.

So far then up the scale of temperature the solar and stellar record is the same; the star at the next stage of heat above the sun has its reversing layer as hot as the sun's chromosphere, and the same "test spectrum" as we have seen fits both. I hold that dissociation simply and sufficiently explains this all-important fact.

But this is as far as the sun can take us. The stars, however, continue the story.

If we consider another change higher up in the scale of temperature, taking as the lower level α Cygni, at which we have arrived, we have independent evidence that the so-called Orion stars are hotter than such a star as α Cygni.

On proceeding to study the higher dissociating temperature at work in the Orion stars, all the statements made with reference to the changes likely to occur in the spectrum on the non-dissociation hypothesis, strictly apply. We cannot expect any change in the relative intensity of the lines and the appearance of the spectrum cannot be fundamentally altered.

On the dissociation hypothesis, on the other hand, if we find certain lines indicating certain substances disappearing, and other lines indicating other substances making their appearance for the first time (or if they were visible before, becoming much intensified), we shall have an opportunity of studying the effects of the new dissociating forces at work.

Now is there any change? The facts are that this increase of temperature we are now considering is accompanied by the gradual extinction of the enhanced lines, an increase in the amount of hydrogen present, and the lines of the cleveite gases, oxygen, nitrogen, and carbon now appear for the first time.

Associating this with the former result, we get as distinct evidence that an increase of the gas lines in the spectrum accompanies the disappearance of the enhanced lines, as that an increased development of the enhanced lines accompanies the decrease of the arc lines.

To take iron as an example, for the sake of simplicity; it will be seen that the actual stellar phenomena might have been predicted up to a certain point, from a consideration of laboratory and solar phenomena. But the stars carry us further than our predictions; we see the gradual increase of hydrogen and cleveite gases. The facts demonstrate that as temperature increases hydrogen increases, and, together with the cleveite gases not obvious before, finally replaces iron which has disappeared.

This is one of the great stellar revelations, and it must be remembered that we have now hundreds of photographs which we can bring together to study the gradual change. There are no "breaks in strata." One of the most wonderful things about this line of work to my mind is the simplicity, coupled with continuity, of the phenomena. It carries conviction with it.

We have then to face the fact that on the dissociation hypothesis, as the metals which exist at the temperature of the arc are broken up into finer forms, which I have termed proto-metals, at the fourth stage of heat (that of the high tension spark) which gives us the enhanced spectrum; so the proto-metals are themselves broken up at some temperature which we cannot reach in our laboratories into other simpler gaseous forms, the cleveite gases, oxygen, nitrogen and carbon being among them.

Does the story end here? No, there is a still higher stage; after the cleveite gases have disappeared as the arc lines and enhanced lines did at the lower stages; the new form of hydrogen to which I have before called attention and which we may think of as "proto hydrogen," makes its appearance. But there are already evidences that even this is not the end of the simplifications brought about by the transcendental stellar temperatures we are now discussing.

It must always be remembered that the Spottiswoode coil (giving a 40-inch spark) with a tremendous battery of condensers only carries up to γ Cygni, by which I mean that using this coil we obtain the enhanced lines of the proto-metals of very nearly the same relative intensities as those under which they appear in that star.

In the stars then we have a few distinct changes of spectra: these changes we know independently by the increase in the length of the spectrum towards the ultra-violet accompany stages of increased temperature. It is most natural to suppose that these increasing temperatures produce increasing simplifications.

Dealing, then, with the changes which we can now study in stellar bodies from the temperature of the sun upwards, we have the series of spectral changes on which the new chemical classification (Chapter VII) has been based.

Now if dissociation is not the cause of these changes where are we to look for one equally simple and sufficient?

It is quite clear that the phenomena to be observed with every increase of temperature, that is in a series of stars with spectra gradually extending more and more into the ultra-violet, must be vastly different if the elements are dissociated from what they would be if the elements remained unchanged.

The only change which we can imagine on the usual hypothesis, as resulting from the increase of temperature, is that with the increase in volume there will be a reduction in density, and all the lines will be equally enfeebled. But this is exactly what does not happen.

It may be said that in consequence of a more exalted temperature in the hottest stars the hydrogen and cleveite gases may, for some reason or other, escape from among the metallic vapours, and form an upper special atmosphere of their own, in which, in consequence of its greater chemical simplicity, the lines of these substances will become more important. But this argument is not philosophical, because we have no right to assume such a change. These gases already exist in the sun and give us no traces of their existence at any great height above the chromosphere; the gas that does exist in these elevated regions is one about which we know nothing, so far, terrestrially, and of which no trace has yet been found in the spectrum of the hottest stars.

I hold, then, that the stars more than justify my appeal to the law of continuity; their verdict is that, as in all previous human experience, a higher temperature brings about simplifications, and it is not strange that as our horizon is expanded by new work we find the same laws in operation. We have, in fact, in these phenomena the work of dissociation carried on before our eyes in the hottest stars, to a point not reached anywhere else, and the stars also tell us that this is possibly beyond our laboratory possibilities, for the highest temperature I have employed only carries us to the heat level of γ Cygni, in which star the cleveite gases, if visible, give only very faint traces. We are thus brought finally face to face with the fact that iron is a compound into the ultimate formation of which hydrogen, or the cleveite gases, or both, may possibly enter.

CHAPTER X.—THE "SERIES" EVIDENCE.

Introduction.

I DEFINED the meaning of the term "Series" on p. 10, and pointed out how one of the important discoveries in recent years enables us to study spectra from a new point of view. I propose in the present chapter to deal with this subject in its most general aspect, and to inquire whether this new method of inquiry helps us with any suggestions or facts which may be utilized in the discussion of the dissociation hypothesis : in other words, whether the new evidence afforded by series, like the new evidence accumulated by the study of stellar spectra, strengthens the view that the line spectra of the so-called chemical elements are produced not by one but by more than one vibrating particle.

To explain what is meant by "series," it is well to begin by studying what are termed fluted spectra. I have already referred to these and given photographs on p. 10; these flutings are perfectly rhythmic from end to end. The whole of a fluting may be regarded as a unit; it is generally strongest towards the right or the red end of the spectrum, its elements gradually becoming dimmer as we approach the violet end. It is well seen in the accompanying untouched photograph of some of the flutings in the spectrum of nitrogen (Fig. 30).

But a fluting is generally more than this; it is built up of subsidiary flutings. Each of the subdivisions of it is in itself an almost exact representatation in the small of what the whole thing is in the great; so that we have the conceptions of a simple fluting and a compound fluting. The compound flutings are well represented in the flutings of carbon and magnesium (*see* Figs. 9 and 10). In all cases we get exquisite rhythm, though in some cases it is apparently overlaid by other lines, and generally the system is intensified towards the red end of the spectrum.

Now when we leave these flutings and study an ordinary line spectrum, in a great many cases all rhythm seems to have disappeared. There is apparently no law and no order. I have already in Fig. 11 given the series observed in the spectra of the cleveite gases. Let us go into this a little closer and compare these "series" with the spectrum as ordinarily observed. Let us take the lines seen when we expose the gas obtained from the mineral cleveite to the action of a strong electric current. We observe no rhythm, and there seems to be a very irregular distribution (Fig. 31).

I may here state that it has always been customary with me in reproducing spectra in the form of illustrations to show the red end of the spectrum on the right hand side and the violet end on the left. As most of the workers on "series" do the opposite, seeing that they have to deal with the *numbers* of waves instead of their *length*, I propose in this chapter to depart from my usual custom

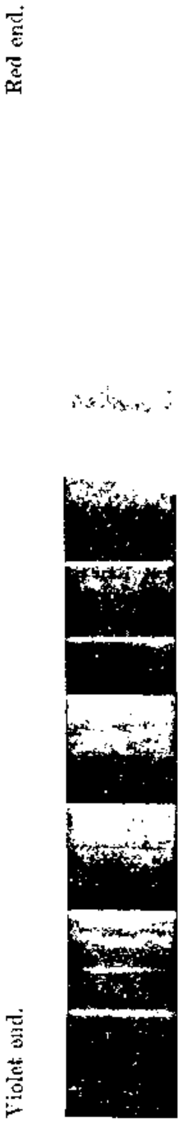

Fig. 30.—Simple flutings of nitrogen.

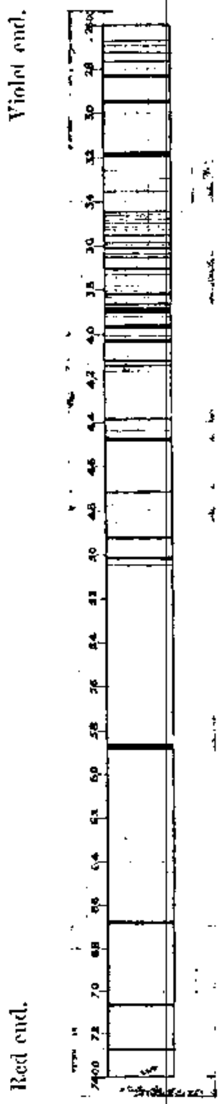

Fig. 31.—Spectrum of the cleveite gases.

and place the red in series spectra on the left, so that all the series illustrations may be comparable *inter se*.

Messrs. Runge and Paschen have shown conclusively that when we come to sort these lines out into series, there is just the same exquisite order that we find in flutings. Fig. 32 shows how they have all been resolved into two sets of three series which gradually get nearer together towards the violet and stronger towards the red; the irregular line spectrum when analyzed in this way, is translated into a wonderful order. I suggested many years ago that

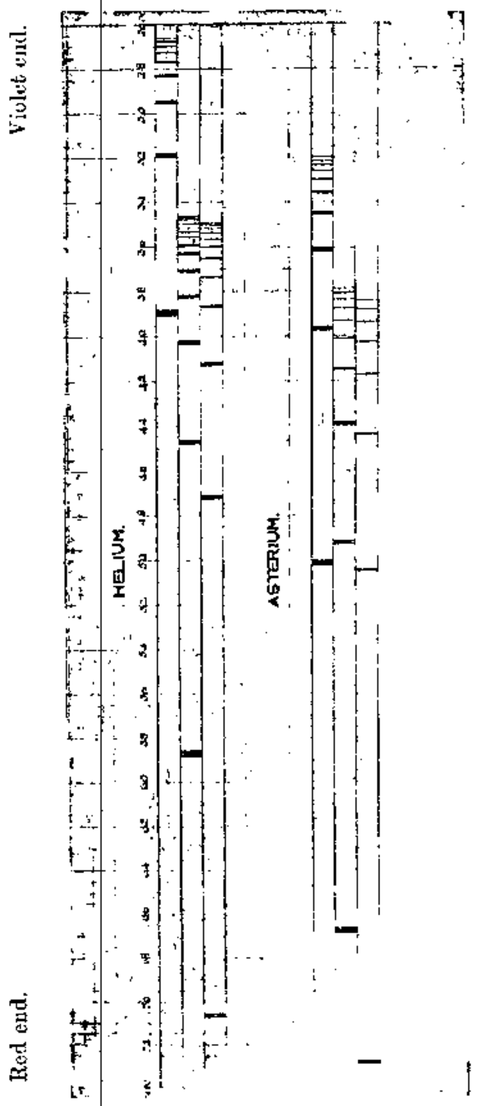

Fig. 32.—Spectrum of the clevite gases sorted out into six regular series.

the triplets in the ordinary line spectrum of a substance may really be remnants of compound flutings, and such inquiries as these really seem to justify that suggestion.

We arrive at the fact that the term "series" applies to related lines. It is impossible to suppose that these wonderful rhythmic series of lines are not related in some way to each other, and that being so we have to study their wave-lengths, that is, their positions in the case of any one element to find out and define the relationship; and not only so, but to see if any relation exists between the lines of different elements.

A Short History.

The history of this quite modern inquiry is not very long, but short as it is I only propose to refer to it in the briefest possible manner.

The first attempt to discover relationships among the lines of spectra was made by Lecoq de Boisbaudran,* who investigated the spectrum of nitrogen. The conclusions he arrived at suggested that the luminiferous vibrations of the molecules could be compared with the laws of sound, but as these were not based on wave-length determinations of sufficient accuracy, and also were not confirmed by Thalén, no great weight could be attached to the result.

Stoney,† who followed up these investigations, was more successful; he showed that the hydrogen lines C, F, and *h* were connected by the relationship 20 : 27 : 32.

Several other workers—Reynolds, Soret, &c.—took the subject up, but it was left for the more thorough work of Schuster‡ to show that this theory could no longer be considered as expressing the law connecting the mutual relationships between the wave-lengths of lines in a spectrum.

Liveing and Dewar§ next called attention to the fact that the distance between two consecutive lines of these groupings decreases with diminishing wave-lengths, so that eventually the lines asymptotically approach a limit. "Harmonic" was the term they used to express such a series of similar groups of lines.

It was, however, the work of Balmer which gave the subject the mpetus by which it has of late years made great progress.

Balmer‖ published a formula by which the positions of the hydrogen lines could be calculated with wonderful accuracy. The formula is as follows:—

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* *Comptes rendus* (1869), vol. lxix, p. 694.

† *Phil. Mag.* (1871), [4], vol. xli, p. 291.

‡ *Brit. Assoc. Report*, 1880; *Proc. Roy. Soc.* (1881), vol. xxxi, p. 337.

§ *Phil. Trans.* (1883), p. 213, and previously.

‖ *Wied. Ann.* (1885), vol. xxv, p. 8.

$$\lambda = \mathrm{A}\frac{n^2}{n^2-4},$$

in which λ is the wave-length in vacuo of the line to be calculated, A constant common for all the lines, and n one of the series of numbers from 3 to 15.

The constant A, according to Cornu's measurements, is 3645·42 Ångstrom units, or, using Ames' more correct value, 3647·20 Ångstrom units.

Simultaneously with Balmer's discovery, Cornu* pointed out that the lines of aluminium and thallium, which are readily reversible, bear a definite relation to those of hydrogen, while at a later date Deslandres† published a formula from which could be calculated the wave-lengths of the lines composing the bands of numerous elements.

The above brief history brings us down to the year 1887, in which Kayser and Runge‡ began their series of minute investigations dealing with a great number of elements. It was also about this time that Rydberg§ commenced to take up the subject.

The work of Kayser, Runge and Rydberg.

I will state generally the ground over which their work has extended. They have attacked the question mathematically from different standpoints. In the following table (p. 88) I give the formula employed by Kayser and Runge, and that employed by Rydberg.

The formulæ are not by any means identical, but both deal with wave-frequency, that is to say, the number of waves in a given unit of length. Both Kayser and Runge, and Rydberg employ certain signs to represent the successive integers which have to be used to define certain of their terms, and in addition to this we get certain constants which are calculated for each series. The most interesting consideration from this point of view is that Rydberg found that there was one constant which he could use in order to search for the series of lines in the spectra of *all* the chemical elements with which

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* *Comptes rendus* (1885), vol. c, p. 1181.

† *Ibid.* (1886), vol. ciii, p. 375; (1887), vol. civ, p. 972.

‡ "Ueber die Spectren der Elemente," *Abhandlungen d. K. Akad. Berlin*, 1888, 1889, 1890, 1891, 1892, 1893.

§ *Svenska Vetenskat. Akad. Handlingar*, Stockholm (1890), vol. xxiii. No. 11; *Wied. Annalen* (1893), vol. l, p. 629; (1894), vol. lii, p. 119.

Formulæ for Calculating Series.

Kayser and Runge.

$$\frac{1}{\lambda} = A + Bn^{-2} + Cn^{-4}$$

where

λ = wave-length

(or

$\frac{1}{\lambda}$ = wave frequency)

$n = 3, 4, 5, \ldots$

A, B, C = constants calculated for each series.

The constants for the principal series are different from those used in the subordinate series.

For sub-series of every element the constant A is nearly identical. For all series of all elements the constant B does not vary by more than 22 per cent. This constant B corresponds to Rydberg's N_0.

Rydberg.

$$n = n_0 - \frac{N_0}{(m+\mu)^2}$$

where

n = wave frequency

$m = 1, 2, 3, \ldots$

N_0 = 109721·6 (a constant applicable to all series of every element)

$\left.\begin{matrix} n_0 \\ \mu \end{matrix}\right\} = \left\{\begin{matrix}\text{characteristic constants} \\ \text{varying with each series.}\end{matrix}\right.$

In the above formula, when $m = \infty$, $n = n_0$; or n_0 is the limit which the number of waves n approaches when m is infinite.

The value of N_0 is assumed by Rydberg to be constant, as it varies only slightly, and this variation may be due to uncertain data.

he worked. There was no common constant similar to this used by Kayser and Runge, but they found that some of their constants varied little from element to element. In that way they not only obtained the first term of a series, but the whole series throughout the entire length of the spectrum, and where observations had been made in the case of the different elements they could of course check their calculations by the actual observations so made, and see how the theory seemed to be justified as the work was extended. The first line in a series must be considered to be comparable to a fundamental note in music. It represents really the longest light wave in the same way that the fundamental note in music represents the longest sound wave. Both series of results, obtained in the way I have described by Kayser and Runge and by Rydberg, show us that, in many cases, we may be almost certain to obtain from the higgledy-piggledy arrangement of the lines in the spectrum of any one substance two or three beautiful regular series like those already shown in the case of the cleveite gases. There is a little difference in the nomenclature employed by the investigators to whom I have referred, as shown in the annexed table.

Series Nomenclature.

Intensity.	Kayser and Runge.	Rydberg.
Strongest	Principal series	Principal series.
Weaker	1st subordinate series	Nebulous series.
Weakest	2nd subordinate series	Sharp series.

The strongest lines which they observed at the temperatures they worked with, they put into what they call a "principal series," and then the weaker lines were distributed among other two series. Kayser and Runge called them the "first-" and "second-subordinate" series; Rydberg calls them the "nebulous-series" and the "sharp-series." The lines of the principal series almost always reverse themselves very easily indeed—that is to say, that the absorption is indicated by them more readily than it is by the other lines. When we come to the second subordinate or sharp series, it is found that these sometimes broaden out towards the red end of the spectrum.

This work, of course, has required considerable investigation; the first attempts were not quite satisfactory, because the observations on which they were based had not been of sufficient accuracy. With greater dispersion it has been found that some of the lines which were supposed at first to be single are really double; so that it is quite usual now when we consider this question of series to suppose that in some cases the series are composed of single lines, in other cases of doubles, and in other cases of triplets; and it was at first, indeed, imagined that in these differences we were face to face with a very important physical difference between the various elements, but Rydberg has suggested that possibly after all it may be a difference merely in the seeing.

He says:*

"The difference between the doubles and triplets is only relative. This opinion is confirmed by the fact that the triplets appear often in the form of doubles, the most refrangible component not having sufficient intensity to become visible. Further, the relative intensity of the components of the doubles seems equal to that of the two less refrangible components of the triplets.

"For these reasons I have dared to propose the hypothesis that the two kinds of component rays are of the same order, or that the doubles are only triplets of which the most refrangible component is too feeble to be seen, or has perhaps the absolute value of zero. . . ."

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* *Kon. Sv. Vet. Ak. Hand.*, vol. xxiii, ii, p. 135.

If the lines are more difficult to see, and if the sub-series of lines get stronger towards either the red end or the blue end, then we are more likely to see one line than two, and more likely to see two lines than three.

With regard to this suggestion made by Rydberg, it is interesting to note that Professor Kayser is not inclined to hold the same opinion and does not look upon triplets, doublets, or single lines of the series as remnants of flutings, whose other members are too weak to be seen. He points out that we have for the elements of the first vertical column in Mendeléeff's table, doublets; for the second column, triplets; for the third, doublets. As the first column contains monavalent elements, the second bivalent ones, the third trivalent, it seems as if the elements with uneven valencies had doublets, those with even valencies triplets. This is confirmed by the triplets of oxygen, sulphur, and selenium, which belong to the sixth column, with even valency. As in every natural group of elements, the first elements show the series strongest, and they get weaker as the atomic weight increases (*i.e.*, in the group of alkalies we cannot see the weaker second series for rubidium and cæsium; in the group copper, silver, gold we can find no series in gold; in the group of magnesium, calcium, strontium, barium, for strontium the second series is already weak, for barium we cannot find the series). We should expect to find, according to Rydberg's hypothesis, in the spectra of every group first triplets, then doublets, then single lines. But that is not so: so long as we find anything of the series the members are and remain triplets or doublets.

There is only a very small number of the chemical elements which give us single lines; in the principal series, so far, we only know of helium and asterium; in the subordinate series we only know of asterium. The number of doubles is very much greater, but it is not so great in relation to the principal series as it is in the case of the subordinate series; but although we have nine elements giving us triplets in the subordinate series, we have only three which give them in the principal series. These results are shown in the following table.

It is well that I should indicate the basis of these statements, and for this purpose I give in Fig. 33 a very small part of the spectra of three different elements, in order that the way in which the work has been done may be followed. In the lower horizon we are dealing with zinc, and the way in which the triplets have been picked out will be easily gathered. The triplet in each case has its central line nearer to one side of the triplet than the other. All the triplets in the zinc spectrum are perfectly symmetrical from that point of view. If we take the upper spectrum—that of calcium—we find also that the

Single lines.		Doubles.		Triplets.	
Principal series.	Subordinate series.	Principal series.	Subordinate series.	Principal series.	Subordinate series.
Helium			Helium	Oxygen	Oxygen
Asterium	Asterium	Hydrogen(?)	Hydrogen	Sulphur	Sulphur
		Lithium (?)	Lithium (?)	Selenium	Selenium
		Sodium	Sodium		Magnesium
		Potassium	Potassium		Calcium
		Rubidium			Strontium
		Cæsium			Zinc
			Copper		Cadmium
			Silver		Mercury
			Aluminium		
			Indium		
			Thallium		

triplets are formed in exactly the same way. We can thus appreciate the enormous labour which has been faced by the inquirers I have named in working out from the spectra of a great many substances and from all the different regions of the spectrum, visible and photographic, these delicate triplets. In a great many cases they do not represent the strongest lines, those most easily seen, and some want a great deal of looking for.

These investigations show that in some cases the series have reproduced the same chemical group, but in some instances the series groupings, so to speak, are quite different from the chemical groupings.

The facts so far ascertained are as follows:—

Group 1	..	Lithium, sodium, potassium, rubidium, cæsium.
,, 2	..	Copper, silver, (gold ?).
,, 3	..	Magnesium, calcium, strontium.
,, 4	..	Zinc, cadmium, mercury.
,, 5	..	Aluminium, indium, thallium.

In the group of lithium, sodium, potassium, the series sequence follows absolutely the chemical sequence. But when we come to the chemical group—calcium, strontium, barium—we find it replaced by a group, magnesium, calcium, strontium, while barium is not used at all. That is a very remarkable departure, and it shows that we have to consider the various conditions which we observe in passing from group to group.

From *group* to *group* with increasing atomic weights the series advance towards the violet. Thus, as the limit of a series is repre-

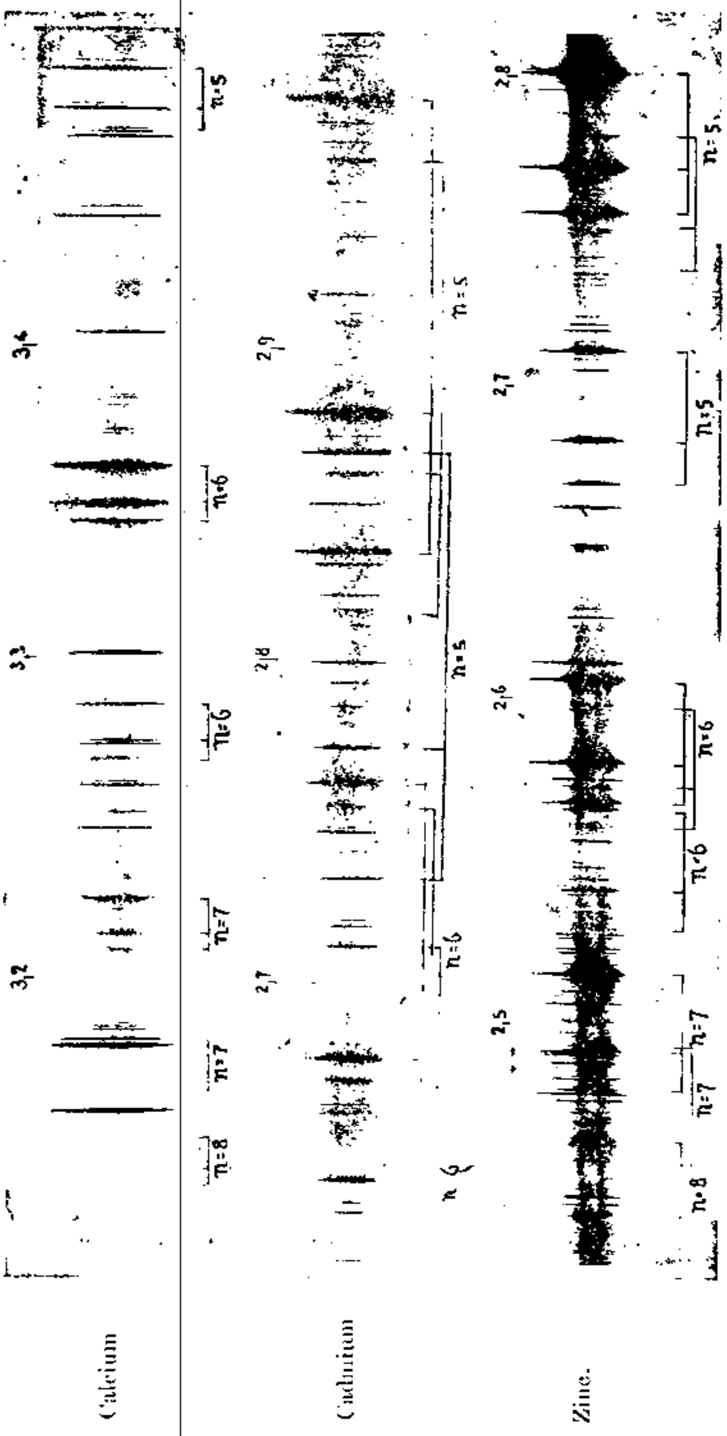

FIG. 33.—Parts of the spectra of calcium, cadmium, and zinc showing the triplets.

sented by the first constant of the first subordinate of the four groups, the theoretical wave-length limit lies

Between 3498·2 and 5065·1 for lithium, sodium, potassium, rubidium, cæsium.
„ 3168·6 „ 3256·0 „ copper, silver, gold.
„ 2512·8 „ 3222·6 „ magnesium, calcium, strontium.
„ 2328·5 „ 2490·1 „ zinc, cadmium, mercury.

In *each group* with the increasing atomic weight the spectrum advances continually towards the red end; also the distance between the components of the doublets and triplets increases with the atomic weights, so that for every group the distance is approximately proportional to the square of the atomic weight.

The Irregularities observed.

The above account I trust will give a general idea of the new investigation in its most general aspect.

I have next to point out that we meet with most marvellous irregularities. We have some elements with many series, in others no series have been detected, the numbers of the series varying even in the gases. With regard to the metals, Kayser has suggested that the melting point seems to have something to do with the phenomena observed; that is, that the higher the melting point the smaller generally is the percentage of lines which is possible to distribute into series. The following table will show this:—

Relation of Series to Melting Points.

Element.	Melting point. Centigrade.	Percentage of series lines.
Barium	1600°	0
Gold	1200	4
Copper	1050	6
Silver	960	26
Strontium	700	20
Calcium	700	34
Magnesium	600	64
Zinc	410	80
Cadmium	320	50
Lithium	180	100
Sodium	90	100
Cæsium	62	100
Potassium	58	100
Rubidium	38	100
Mercury	−40	27

The accompanying general table will show the facts touching these various points which are at present known. The metallic elements are arranged in the order of Mendeléeff's groups, and the irregularities touching the total number of series, of principal series, the simple or compound nature of the lines of each series, and percentage of lines picked up by the various series, can all be gathered from an inspection of the table.

	Mendeléeff groups.	Atomic weights.	No. of series.	No. of principal series.	Principal series.			1st and 2nd subordinates.			Per cent. of total number of lines picked up by K. and R.	Per cent. of lines of intensity 10, picked up in series.	Melting points, degrees Centigrade.
					Single.	Double.	Triplet.	Single.	Double.	Triplet.			
Hydrogen	..	1	3	1	..	?	..	..	×	..	100	100	..
Helium	..	..	3	1	×	..	..	..	×	..	100	100	..
Asterium	..	..	3	1	×	..	..	×	..	..	100	100	..
Lithium	I.	7·0	3	1	..	?	..	..	?	..	100	100	180
Sodium		23·0	3	1	..	×	..	..	×	..	100	100	90
Potassium		39·0	3	1	..	×	..	..	×	..	100	100	58
Rubidium		85·2	1	1	..	×	..	..	?	..	100	100	38
Cæsium		13·3	1	1	..	×	..	..	?		100	100	62
Copper	I.	63·4	2	0	..	..	..	..	×	..	6	..	1080·5
Silver		107·6	2	0	..	..	..	..	×	..	26	..	960
Gold		196·7	0	0	..	..	..	..	..	..	?	..	1061·7
Magnesium	II.	24·3	2	0	..	..	..	..	..	×	64	55	600
Calcium		39·9	2	0	..	..	..	..	..	×	34	17	700
Strontium		87·4	2	0	..	..	..	..	..	×	20	7	700
Barium		136·8	0	0	..	..	..	..	..	..	..	..	475
Zinc	II.	65·1	2	0	..	..	..	..	..	×	80	43	410
Cadmium		111·7	2	0	..	..	..	..	..	×	50	14	320
Mercury		199·8	2	0	..	..	..	..	..	×	27	12·5	−40
Aluminium	III.	27·0	2	0	..	..	..	..	×	..	..	25	654·5
Indium		113·7	2	0	..	..	..	..	×	..	..	25	176
Thallium		203·7	2	0	..	..	..	..	×	..	..	17	282
Tin	IV.	117·8	0	0	..	..	..	..	..	..	..	..	232
Lead		206·4	0	0	..	..	..	..	..	..	..	..	326
Arsenic	V.	74·9	0	0	..	..	..	..	..	..	..	..	450
Antimony		119·6	0	0	..	..	..	..	..	..	..	..	629·5
Bismuth		207·5	0	0	..	..	..	..	..	..	..	..	270
Oxygen	VI.	15·88	6	(2)	..	..	×	..	..	×	..	..	..
Sulphur		31·8	3	1	..	..	×	..	..	×	..	..	114
Selenium		78·5	3	1	..	..	×	..	..	×	..	..	217

With regard to the stated absence of "principal series" in the case of zinc, cadmium, and mercury, it may be pointed out that in each case a very strong broad reversed line in the ultra-violet may represent the principal series; and in the case of copper, silver, and gold, each of these elements contains in the ultra-violet a very strong pair of lines which may represent the principal series.

I think it is quite fair to remark at this stage of our inquiry, that if all the vibratory atoms which produce the spectra of the chemical elements had all been brought to a similar condition of greatest simplicity, in other words, if we were really dealing with the chemical atom as defined, in each case, the amazing irregularities which we have found could hardly be expected.

Some Details.

I will next go a little further into detail in the case of some elements for the sake of instituting comparisons, and seeing whither the results lead us.

The most remarkable case which I have to refer to is that of hydrogen. We do not know the meaning of it yet, but it has to be taken into account in any consideration of these questions. Until a little time ago only one series was known in the spectrum of this gas, and reasoning on this basis, it was thought that the atom of hydrogen was far more simple than that of any other chemical element, and also that a chemical atom was only competent to produce one series. A short time ago, however, Professor Pickering, in his magnificent work on the stars, to which I have already had the opportunity of referring, pp. 58 *et seq.*, discovered a second series of lines. Not long after, Professor Rydberg suggested that one of the most important lines seen in a large group of stars really represented a line of the principal series of hydrogen. That conclusion has been generally accepted, although the evidence is considered doubtful by some; so that we now assume that hydrogen has three series like helium and asterium, and we seem therefore to be on solid ground in one direction, at all events, in regard to some gases. That is, we may assume either that a simple atom may by vibrating produce three series, or that hydrogen itself is of *at least* threefold complexity. We have another series of metals of low atomic weight, which therefore chemically are supposed to represent a considerable simplicity; we find that in the case of lithium and sodium we also deal with three series, a principal series and two subordinate series. The same remark applies to potassium. It has recently been found that sulphur and selenium also give us three series. We have a principal series and the first and second subordinates, the suggestion of anything beyond these three is confined to one or two lines in each case.

But if we pass from the gas hydrogen to the gas oxygen, what do we find?

In oxygen *we have six series*, that is twice as many as we know of in hydrogen, helium, asterium, lithium, sodium, sulphur, and so on. So far as that goes, we are in the same condition that we were some time ago when we imagined that the gas obtained from the mineral eleveite, when exposed to the action of a high tension spark, was really a single gas with six series. Very many arguments have been employed to show that that view is probably not an accurate one; so that some are prepared to separate the cleveite gas *at spark temperatures* into two, calling one helium and the other asterium. That brings these two constituents of the cleveite gas, brought out by high temperatures, to the same platform as hydrogen with the recent developments, lithium, sodium, sulphur, &c.

If we consider this extraordinary condition in the case of oxygen a little further, *we find that the six series only after all pick up the oxygen lines seen at a low temperature*, and that if we employ a high temperature to observe the oxygen spectrum, that is to say, if we use an induction coil, a jar and an air break, we find a very considerable number of lines which have no connection whatever with any of the series so far made out. And we are face to face with this very awkward fact, that in the case of oxygen there are more lines which we cannot get into a series than there are lines in the six series which we have attributed to that chemical substance. Here, therefore, on the hypothesis that we are dealing with the oxygen "*atom*," we begin certainly to get into difficulties. The inquiry is not straightforward.

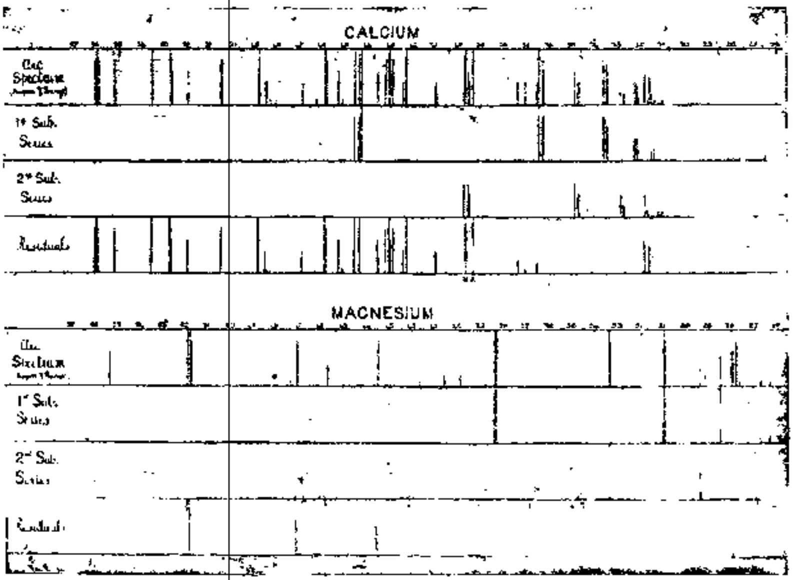

FIG. 34.—Map showing series and residual lines in spectra of calcium and magnesium.

The next point is, that in the case of other substances, *we have no principal series*, but only two subordinate ones. This happens in the case of magnesium, calcium and strontium, and also aluminium, zinc, and tellurium; we have a first and second subordinate series, but no principal series. I have studied the lines of calcium and magnesium, in the same way that the lines of oxygen were studied, to see how many of the lines are picked up by the series, and I proceed to furnish some details. In the upper part of the diagram (Fig. 34) I give the lines seen in the arc spectrum of calcium, and in the two next horizons we have the lines picked up in the first and second subordinate series. The next horizon gives the residual lines—lines, that is, which are not distributable among these series. We see that there is a large number outstanding just as in the case of oxygen, and it is very important indeed to note that the two lines H and K, which are more conspicuous in the spectrum of the sun than all the other lines of the spectrum, have not been caught by any of these researchers into the series of calcium. Therefore, with a reduced number of series, we seem to be getting still further from the simplicity we began with in the case of some of the permanent gases like hydrogen and helium. The same thing holds with regard to magnesium, the spectrum of which at the temperature of the arc has not so many lines in it as the spectrum of calcium. A certain number of these lines has been picked up to form the series, but we get numerous lines which have been left over after all attempts to sort them into series have been made.

I have now to refer to another consideration. We have dealt so far in the case of calcium and magnesium with *arc* temperatures, but I showed on pages 35 and 36 that in the case of these metals at *spark* temperatures, the spectra are greatly changed, enhanced lines making their appearance; and I stated on page 57 that the all-important lines in the hottest stars are lines seen at the temperature of the spark. I have added these lines to the diagram, and we see that there is not the slightest trace of those lines having been picked up in the series. So that the further we go, the more we seem to get away from that beautiful simplicity with which we began.

I refer next to another group of substances, namely, tin, lead, arsenic, antimony, bismuth and gold, and I might mention more. No series whatever have as yet rewarded the many attempts of those who have tried to get those metals and non-metals on all-fours with those previously investigated. As already stated, it remained for Kayser and Runge to point out that it looked very much as if this complete absence of series was connected with the melting points of the substances with which they had been dealing. So long as the melting point was low, as in the case of sodium and lithium, the normal three series would show at low temperatures; and, further, there were no lines over. But, when we deal with substances with high melting points, there are no series at all. In the case of lithium, sodium, potassium, &c., all the lines are picked up: in the case of copper, silver, and gold, the series pick up only a very small proportion. There seems, therefore, to be a progression of complexity with the increasing melting point with regard to all the metallic substances which have so far been examined.

In the case of barium with a high melting point, we get no lines at all represented in "series"; contrasted with 100 per cent. in the case of lithium. But then again, when we come to mercury, which is also of low melting point, instead of getting 100 per cent. we only get about 25 per cent. of the lines represented. The metals then vary as do the gases.

General Conclusions.

The evidence then seems to indicate that the chemical units in the case of the elements studied by the movements written out by these series must possess different degrees of complexity. A little time ago it was imagined that hydrogen was rendered visible to us by such simple vibrations that only one series of lines could be produced. If that is so, then it looks very much as if whenever we see three series of lines *at least* three molecules or atoms, three different things, are in all probability at work in producing them. When we get six series, that points to a still greater complexity, and when, as in the case of oxygen, we get six series not accounting for half the lines, then we should be quite justified, I think, in supposing that oxygen was one of the most complex things that we were brought face to face with in our studies of "series" in cases where they are observable. When we come to metals where there are no series at all, what do we find? We are dealing with substances with high melting points—that is to say, we cannot bring them down easily to those mobile states represented by the free paths and collision conditions of a permanent gas; and it is quite easy to suppose, on that account alone, that we do not see the vibrations of any of the more simple forms.

Hence, then, I submit that the evidence presented as to the complex origin of line spectra by the studies of "series" is as clear as that obtained from high temperature work in the laboratory and a discussion of stellar spectra in relation to that work.

I have already referred to the case of hydrogen.

Professors Pickering and Kayser both concede that the new series is due most probably to a high temperature, and Kayser expressly states, "that this series has never been observed before, can perhaps be explained by insufficient temperature in our Geissler tubes and most of the stars."

It seems as if the two series are of the "subordinate" type, and that the principal series is wanting if Rydberg's conclusion be not accepted; because while in subordinate series the lines for large values of n lie very near to one another, the similar lines of the principal series on the other hand are always more refrangible. It seems, therefore, probable that one or two of the many unknown lines recorded in stellar spectra still awaiting identification may belong to the principal series of hydrogen.

If we are dealing in this case with a single molecule of hydrogen vibrating in a previously unknown way in consequence of a higher temperature, why is it that the molecules of other bodies do not put on similar transcendental vibrations and appear in the same stars so that we shall get new forms of the other chemical elements? The fact that we do not do so is, I claim, an argument in favour of the view that the principal and subordinate series are produced by molecules of different complexities, and that the finer molecules can alone withstand the action of the highest temperatures, and require high temperatures to produce them.

In this way we can easily explain the visibility of the new form of hydrogen in connection only or mainly with the lines of the cleveite and other similar gases (for there is already evidence of the existence of other similar gases) in the hottest stars.

From the admirable work done on such substances as lithium, sodium and potassium, which apparently are reduced to their finest atoms at relatively low temperatures, and more recently on the series of oxygen seen at low temperatures, we are bound to consider that when the research includes the complicated spectrum of iron that that also must follow suit; but it is already obvious that a principal and two subordinate series will never do; there will be very many series involved.

Now these series must include both the arc and the enhanced lines, and as these are visible each without the other in stars of different temperatures, in one case associated with the cleveite gases, in another without them, we have another argument in favour of molecular complexity.

I may here point out that it is always the hot line which avoids "series." The argument that lines in series represent the vibration of one molecule proves that lines not in series are produced by the vibrations of some other molecule.

Finally then, I stated in 1878 that the spectrum of a substance was the integration of the spectra of various molecular groupings.

It has now been definitely established that the spectrum of some substances is the integration of "series."

So far there has been no definite pronouncement touching the possibility that each series may represent vibrations of similar molecules, but the facts as they stand are in favour of this view so long as we consider a series as representing the simplest result of atomic vibration. There are facts which suggest that even a series is not a simple result.

I am glad to be able to complete this chapter, which Professor Kayser has kindly read over for me, with the following expression of his opinion, which he allows me to publish.

"I quite agree with your opinion, that the molecules of elements are in general very complicated systems of atoms, and that their complexity is very variable with temperature and perhaps other conditions. I think that at the highest temperature every molecule has the simplest structure; is perhaps a single atom; and that in this condition it will emit a very simple spectrum consisting of one, or perhaps three, series of doublets or triplets. If the temperature is not high enough above the melting point to dissociate all the molecules, nevertheless some will be dissociated, and we shall have always a mixture of molecules, from the most complex ones that can exist at this temperature to the most simple ones. When the temperature gets lower and lower, more and more complex molecules will be added, while the simplest ones gradually disappear. In the same degree the simplicity of the spectrum is lost, of the series only the strongest lines or none remain, and the spectrum is the sum of more or less lines of a great many different spectra. I expressed the same opinion in the first publication of Kayser and Runge (*Abhandl. d. k. Akad., Berlin*, 1888), and I think our researches have shown nothing that contradicts it."

CHAP. XI.—EVIDENCE AFFORDED BY THE SHIFTING OF LINES.

RECENT work in America, by means of the great dispersion afforded by Rowland's concave gratings, has supplied us with results* of the highest interest, touching small variations in the wave-lengths of spectral lines and the causes which produce them. These are stated to have been, in the first instance, established by Mr. Jewell by an examination of the Rowland series of photographs of the solar and metallic spectra taken by means of a concave grating of 21½ feet radius and 20,000 lines to the inch—an instrument of research which, so far as my own experience goes, is obtained with great difficulty by workers in this country.

Mr. Jewell's investigations began in 1890. Messrs. Humphreys and Mohler studied in 1895 the effects of pressure on the arc spectra of the elements, work suggested by Mr. Jewell's prior researches.

Mr. Jewell, as a basis for his new conclusions, investigated under modern conditions classes of phenomena which I was the first to observe and describe more than a quarter of a century ago.

To show the relation of the new work to the old, it is best to begin with a short historical statement, which will have the advantage of giving an idea of the meaning of some of the terms employed.

I first employed, as stated on p. 22, the method of throwing an image of a light source on to the slit of a spectroscope by means of a lens in 1869, and some of the results obtained by the new method were the following.

(1) The spectral lines, obtained by using such a light source as the electric arc, were of different lengths; some only appeared in the spectrum of the core of the arc, others extended far away into the flame and outer envelopes. This effect was best studied by throwing the image of a horizontal arc on a vertical slit. The lengths of the lines photographed in the electric arc of many metallic elements were tabulated and published in *Phil. Trans.*, 1873 and 1874.

(2) The longest lines of each metal generally were wider than the others, the edges fading off, and they reversed themselves; by which I mean that an absorption line ran down the centres of the bright lines. These results were afterwards confirmed and extended by Cornu.†

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* *Astrophysical Journal,* February, 1896, vol. iii, p. 114.

† *Chemistry of the Sun,* p. 379.

(3) From experiments with mixtures of metallic vapours and gases it came out that the longest lines of the smaller constituent remained visible after the shorter lines had disappeared, the spectrum of each substance present getting gradually simpler as its percentage was reduced,[*] the shorter lines being extinguished gradually. Shortly after these observations were made, I included among some general propositions:[†] "In encounters of dissimilar molecules the vibrations of each are damped."

(4) The various widths of the lines, especially of the winged longest ones, were found to depend upon pressure or density, and not temperature.[‡]

(5) The "longest lines" of any one metal were found to vary in their behaviour in most extraordinary fashion in solar phenomena, being furthermore differentiated from the shorter ones; and on this and other evidence, I founded my working hypothesis of the dissociation of the chemical elements at the solar temperature. In 1876 I set out the facts with regard to calcium.

(6) In 1883, Professor W. Vogel, in a friendly criticism, pointed out the evidence, then beginning to accumulate, that under certain circumstances the wave-lengths of lines are changed.[§] In 1887, I extended this evidence, and I think it was I who coined the word "shift" to express these changes.[¶]

I now pass on first to the results which Mr. Jewell claims to have established.

With the enormous dispersion produced by the instruments referred to, it is found that certain metallic lines, but not all, are displaced or "shifted" towards the violet when compared with the corresponding solar lines. "There was a distinct difference in the displacement, not only for the lines of different elements, but also for the lines of different character belonging to the same element."

The "different character" above referred to turns out to relate not so much to the intensity as to the length of the line, and, associated with this, its reversibility; the longest lines are the most displaced, the shortest, least.

Further, in the spectrum of the arc itself, the position of a line with but little material present "was approximately the same as the position of the line when reversed." Now since the longest lines are most displaced to the violet, this means that the smaller the quantity of a substance present the greater is the displacement towards the violet; and, therefore, the greater the quantity present, the greater the displacement towards the red.

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[*] *Phil. Trans.* (1873), p. 482.

[†] *Studies in Spectrum Analysis* (1878), p. 140.

[‡] *Phil. Trans.*, 1872, p. 253.

[§] *Nature*, vol. xxvii (1883), p. 233.

Chemistry of the Sun, p. 369.

[¶] Since the parentage is uncertain, I may say that perhaps "shiftings" would have been a better word, as shift is otherwise employed, *e.g.*, Love's last shift (translated by a French author, *la dernière chemise de l'amour*).

Mr. Jewell found that "with an increase in the amount of the material in the arc there was an increasing displacement of the line towards the red," and then that, "unless the line became reversed, all further progress in that direction ceased."

Here is an observation regarding the red line of cadmium. "It was found that if the micrometer wires were set upon it with very little cadmium in the arc, then as the amount was increased the line almost bodily left the cross-hairs, always moving towards the red."

Mr. Jewell considers he has established that the vibration-period of an atom depends to some extent upon its environments. "An increase of the density of the material, and presumably an increase of pressure, seemed to produce a damping effect upon the vibration period." My result of 1872 with regard to pressure was endorsed, "the new results are found to be due to pressure and *not* temperature."

We seem, then, now to be in presence of two damping effects in the case even of metallic lines, one which extinguishes lines when we deal with dissimilar molecules, and one which changes their wave-length towards the red when we deal with similar molecules.

A carefully prepared table showed the origin, intensity and character of the solar lines considered, the intensity and character of the corresponding metallic lines, the wave-lengths of both, and the observed displacement.

Many references to solar phenomena were made by Mr. Jewell in relation to his work, but I do not propose to discuss them here. There is one point, however, I must refer to. He considers that the conclusions to be drawn from a study of the new shifts "effectually disposes of the necessity of any dissociation hypothesis to account for most solar phenomena." I have already pointed out that this was Professor W. Vogel's conclusion with regard to possible shifts, so far back as 1883.

It is quite easy. "Two adjacent lines of iron, for instance, may show the effects of a violent motion of iron vapour in opposite directions, in the neighbourhood of spots, or one line (the smaller one corresponding to one of Lockyer's 'short lines') may show a broadening and increase of intensity in the spectrum of a sun-spot, while the other line (the larger one corresponding to one of Lockyer's 'long lines') is unaffected. But this does not prove that iron vapour is dissociated in the sun. It merely shows that the apparently similar portions of the two lines in the solar spectrum are produced at different elevations in the solar atmosphere. The stronger iron line will be affected in a sun-spot as much as the other one, but it is the portion of the line produced at the same level as the other line, and may be masked completely, or very largely, by the emission line produced at a higher level, while the second absorption line in the solar spectrum may be entirely unaffected, being produced at a still higher altitude."

"This also explains why some of the lines (the short lines generally) of an element may be most prominent in sun-spot spectra, while others (generally the long lines) are those most frequently seen in prominences or in the chromosphere."

My thirty-three years' work at solar physics leaves me with such an oppressive feeling of ignorance that I willingly concede to Mr. Jewell a knowledge so much greater than my own as to give him a perfect right to dismiss all my work in two lines; but I am compelled to point out that he has not carefully read what I have published.

A comparison of the facts brought together on page 26, for instance, drives his last paragraph into thin air; it is distinctly shown that we have to do with the short lines in the chromosphere and with the long lines in spots, the exact opposite of his statement. Mr. Jewell does not run counter to my views in supposing that different phenomena are produced at different elevations. I thought I had abundantly proved in my eclipse observation of 1882 (*Chemistry of the Sun*, p. 363), and the later evidence will be found on p. 41, *et seq.*, that the iron lines, to take a concrete instance, are produced at different heights in the solar atmosphere; and that was one among many reasons which compelled me to abandon the thin reversing layer suggested by Dr. Frankland and myself in 1869 in opposition to Kirchhoff's view. But surely the more we consider the solar atmosphere as let out in flats, with certain families of iron lines free to dwell in each and to flit *à discrétion*, the more a dissociation hypothesis is wanted. And beyond all this, we have to take into account that at the sun-spot maximum no iron lines at all are seen amongst the most widened lines, while at the minimum we have little else.

The real bearing of the new work on the dissociation hypothesis has been accurately caught by Professor Hale, as I shall show later.

Another very interesting part of Mr. Jewell's work refers to the phenomena of absorption. There is room for plenty of work here. As I pointed out in 1879, we get unequal widenings, "trumpetings," and a whole host of unexplained phenomena.* It is clear that the

* *Chemistry of the Sun*, pp. 380—387.

enormous dispersion at Mr. Jewell's command will largely help matters.

I now pass to Messrs. Humphreys and Mohler's researches.

These investigators used an electric arc enclosed in a cast-iron cylindrical vessel, which enabled them to vary the pressure up to fourteen atmospheres. One hundred photographs of metallic spectra were taken, and the shifts of some lines of twenty-three elements have been measured. The accompanying rough diagram, bringing together specimens of their observations, will indicate the kind of result they have obtained.

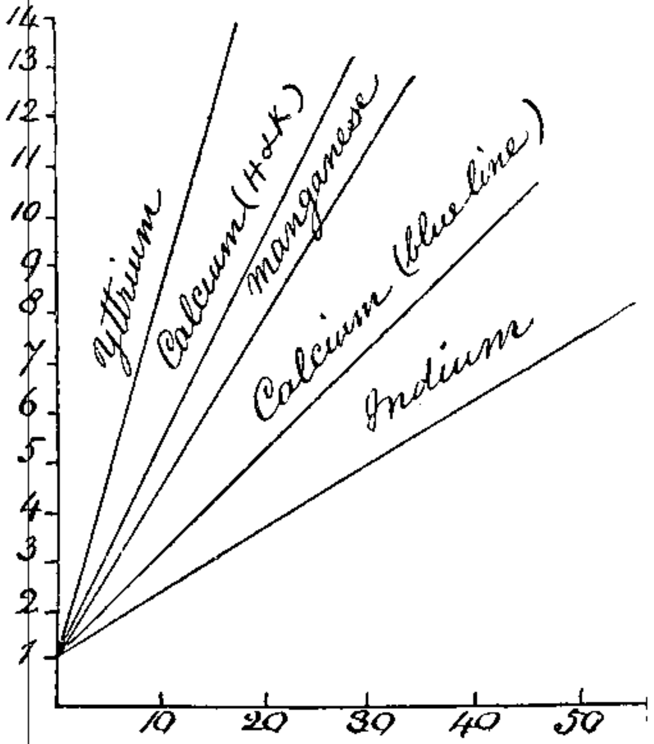

FIG. 35.—Changes of wave-length produced by pressure, showing the different behaviours of the lines of calcium (H and K and the blue line).

The pressures in atmospheres are shown to the left. The shift towards the red in thousandths of an Angström unit are shown below. The shifts have been reduced to what they would be at λ 4000, in the neighbourhood of which most of the work was done.

The displacement or shift varied greatly for different elements. It was always towards the red, and directly proportional to the wavelength and the excess of pressure over one atmosphere.

Only one exception to this general statement was noted at the beginning of the inquiry; it refers to calcium. "The lines H and K, among others, shift only about half as much as *g* (the blue line at λ 4226·91), and the group at λ 5600. That *g* should differ in this respect from H and K is not very surprising, since it is known to differ greatly from them in many other respects."

On this exceptional behaviour of these lines of calcium, I quote the following, from a note by Professor Hale.*

"The difference in behaviour of H and K and the blue line of calcium discovered by Messrs. Jewell, Humphreys, and Mohler, seems to support Lockyer's views as to the dissociation of calcium in the arc and sun. The remarkable variations of the calcium spectrum with temperature have long been known principally through the investigations of Lockyer. The writer has shown that the H and K lines are produced at the temperature of burning magnesium and in the oxy-coal-gas flame. They could not be photographed in the spectrum of the Bunsen burner, though an exposure of sixty-four hours was given. Since these experiments were made, I have been informed by Professor Eder that his own efforts to photograph the lines in the Bunsen burner were no more successful, though an optical train of quartz and fluor-spar was employed. It would thus appear that the temperature of the dissociation of calcium is between that of the Bunsen burner and that of the oxy-coal-gas flame. The high molecular weight of calcium has hitherto conflicted with our belief in the presence of this metal in prominences. If, however, it be granted that dissociation can be brought about by temperatures even lower than that of the arc, the difficulty is very greatly lessened."

In an article which I wrote in *Nature* on this work,† I pointed out that "it would be very interesting to see if the strontium line at λ 4607·52 behaves like the calcium *g* in relation to the lines at λ 4077·88 and λ 4215·66, representing H and K."

This prediction was subsequently confirmed by Mr. Humphreys,‡ who gave a table of the shifts measured on the strontium lines mentioned above. When working with pressures varying from 6 to 12 atmospheres, the shift of the line at λ 4077·88 was always approximately half that at λ 4607·52.

There can be little doubt after this successful prediction that other enhanced lines will follow suit as this new attack is carried further.

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* *Astrophysical Journal*, *loc. cit.*

† *Nature*, vol. liii. p. 416, March, 1896.

‡ "The Effect of Pressure on the Wave-lengths of lines in the Spectra of certain Elements," *Astrophysical Journal*, vol. iv, p. 249.

Artificial Shifting of Lines.

The "shifts" we have so far referred to are real, depending upon the environment of the molecules the vibrations of which build up the spectra.

But there are also what we may term artificial shifts, the observation of which has recently led Dr. Schuster and Mr. Hemsalech to conclusions of great importance almost equalling those noted by Messrs. Jewell, Humphreys and Mohler from our special point of view.

To see the point of this new work, let us consider a strong jar spark taken between two different metallic poles in air. What happens is thus described.

"The initial discharge of the jar takes place through the air; it must do so because there is at first no metallic vapour present. The intense heat generated by the electric current volatilises the metal which then begins to diffuse away from the poles; the subsequent oscillations of the discharge take place through the metallic vapours and not through the air."*

Next let us assume that the vapours produced at each pole *take time* to pass to the other. If we observe by means of a revolving mirror, the spark *quâ* air will give us a straight line, the spark *quâ* each vapour will give us curved lines.

Next suppose that instead of observing the sparks thus produced by the three different sources, we observe their spectra. This has been done by Dr. Arthur Schuster and Mr. Hemsalech, who thus refer to it:—

"The method of the rotating mirror tried during the course of several years in various forms by one of us, did not prove successful. On the other hand good results were obtained at once on trying the method used by Professor Dixon in his researches on explosive waves. This method consists in fixing a photographic film round the rim of a rotating wheel. All that is necessary for its success is to have sparks so powerful that each single one gives a good impression of its spectrum on the film. Were the sparks absolutely instantaneous, the images taken on the rotating wheel would be identical with those developed on a stationary plate, but on trial this is found not to be the case. The metal lines are found to be inclined and curved when the wheel rotates, and their inclination serves to measure the rate of diffusion of the metallic particles. The air lines, on the other hand, remain straight, though slightly widened.

"To avoid the tendency of the film to fly off the wheel when fixed round its rim, as in the original form of the apparatus, a spinning disc was constructed for us by the Cambridge Scientific Instrument Company. The film is placed flat against the disc, and is kept in place by a second smaller disc, which can be screwed lightly to the first. The diameters of the two discs are 33 and 22·2 cm., the photographs being taken in the annular space of 10·8 cm., left uncovered by the smaller disc. An electric motor drives the disc, and we have obtained velocities of 170 turns per second, though in our experiments the number of revolutions was generally about 120, giving a linear velocity of about 100 metres second for that part of the film on which the photograph was taken."

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* *Proc. Roy. Soc.*, vol. 64, p. 331.

Now the curvature of the metallic lines must depend upon the rate of diffusion of the vapours in opposite directions from the metallic poles; and if the spectrum of each metal used as a pole be due to the vibrations of one set of molecules, there will be equal curvature in all the lines of that metal.

The photographs however, so far taken, show that the curvature is *not* equal; so in this work as in the other I have referred to in the previous chapters, and shall refer to in subsequent ones, we are driven to the conclusion that the spectrum has a complex origin. The results of the investigation, so far as it has gone, have not yet been completely published, but Dr. Schuster in a letter to me states that he has "no doubt as to great differences in inclination [curvature] of the bismuth lines. I also believe the difference to be real in the case of the zinc lines (the green doublet being different from the blue triplet), but this I do not consider established with the same certainty as in the case of bismuth."

In order to give an example of the magnitude of the differences in velocity determined by the unequal curvature of the lines, Dr. A. Schuster allows me to print the following numbers:—

Metal.	Wave-length.	Velocity metres second.
Zinc	4925 4912	415
	4811 4722	545
Cadmium..............	5379 5339	435
	5086 4800 4416 3613	559
Bismuth	5209 4561 3696	1420
	4302 4260	533
	3793	394
Mercury	4359	481
	3663	383

CHAP. XII.—EVIDENCE AFFORDED BY THE MAGNETIC PERTURBATIONS OF LINES.

LONG before the present electro-magnetic theory of light was formulated in its present shape, several observers endeavoured to see if any spectrum change was to be noted when the light source was placed in a strong magnetic field.

Of these, Professor Tait seems to have been the earliest. He made the attempt in 1855:[*] it led to no result. The same thing happened to Faraday in 1862. Indeed, his experiment on this question was the last he ever made. I extract the following account of it from his life by Dr. Bence Jones:—[†]

"1862 was the last year of experimental research. Steinheil's apparatus for producing the spectrum of different substances gave a new method by which the action of magnetic poles upon light could be tried. In January he made himself familiar with the apparatus, and then he tried the action of the great magnet on the spectrum of chloride of sodium, chloride of barium, chloride of strontium, and chloride of lithium."

An experiment made on March 12 is thus recorded:—

"The colourless gas flame ascended between the poles of the magnet, and the salts of sodium lithium were used to give colour. A Nicol's polarizer was placed just before the intense magnetic field, and an analyzer at the other extreme of the apparatus. Then the electro-magnet was made and unmade, but not the slightest trace of effect on or change in the lines in the spectrum was observed in any position of polarizer or analyzer.

"Two other pierced poles were adjusted at the magnet, the coloured flame established between them, and only that ray taken up by the optic apparatus which came to it along the axis of the poles, *i.e.*, in the magnetic axis, or line of magnetic force. Then the electro-magnet was excited and rendered neutral, but not the slightest effect on the polarized or unpolarized ray was observed."

About the year 1872, Professor Clifford and myself made some experiments with the large Steinheil spectroscope then in use in my laboratory at the School of Science; the only magnet available was a feeble one, and nothing came of them. In 1885 M. Fievez* was more fortunate. He made a set of experiments which may be said to be the first recorded success, or at least partial success, of the solution of this problem which now concerns us. M. Fievez observing with a flame in a magnetic field as Faraday had previously done. He noticed a widening, and apparently a doubling of lines, but the doubling he attributed to absorption. He wrote:—

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[*] *Proc. Roy. Soc. Edin.*, vol. ix, p. 118, 1875-6.

[†] Vol. ii, p. 449, 1870.

"Les phénomènes qui se manifestent sous l'action du magnétisme sont identiquement les mêmes que ceux produits par une élévation de température."

In spite of this, however, Dr. Preston has expressed the opinion that if Fievez "had known the theory, the whole question would have been settled in 1885."

The subject remained unfruitful until 1897, when Dr. Zeeman made known the results† of an important series of observations which he had been quietly carrying out.

In a course of measurements concerning the phenomena first observed by Dr. Kerr, Dr. Zeeman was led to reopen the inquiry whether the light of a flame submitted to the action of magnetism really did undergo any change. He remarked: "If a Faraday thought of the possibility of the above-mentioned relation, perhaps it might yet be worth while to try the experiment again with the excellent auxiliaries of spectroscopy of the present time. . . ." And his observations established that the bright lines of spectra are modified considerably when a strong magnetic field is used. It was at once seen why previous experimenters had failed: the effect is small, so that besides a strong field, high dispersion is necessary.

No sooner had Dr. Zeeman made his discovery public, than Professor Lorentz, and subsequently Dr. Larmor, investigated the subject theoretically. They showed that dealing with the theory in its simplest form, not only mere broadening of the lines should be expected, but that each line should really consist of three separate lines, or in other words, form a triplet.

According to the simple theory, each element of matter which carries an electric charge proper to it—the complex being called an ion— has its movements affected by the magnetic field.

If we consider these ions to be the elements of matter the movements of which produce light, it is certain that in a magnetic field the movements will be affected; there will not only be the normal movement in the orbit, but an added precessional movement, or spin, round the lines of magnetic force. If we represent the electric charge of the ion by e, and its inertia by m, the ratio e/m in a field of given strength is proportional to the precession, or spin, of the orbit of the ion.

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* *Bulletin de l'Acad. des Sciences de Belgique*, 3e Série, tome ix, p. 381, 1885.

† *Phil. Mag.*, [5], vol. xliii, p. 226.

By using specially constructed electro-magnets, and arranging special conditions of the experiment, it was not long before a magnetic field was produced which was sufficiently strong to completely separate the components of the lines previously thought to be only broadened.

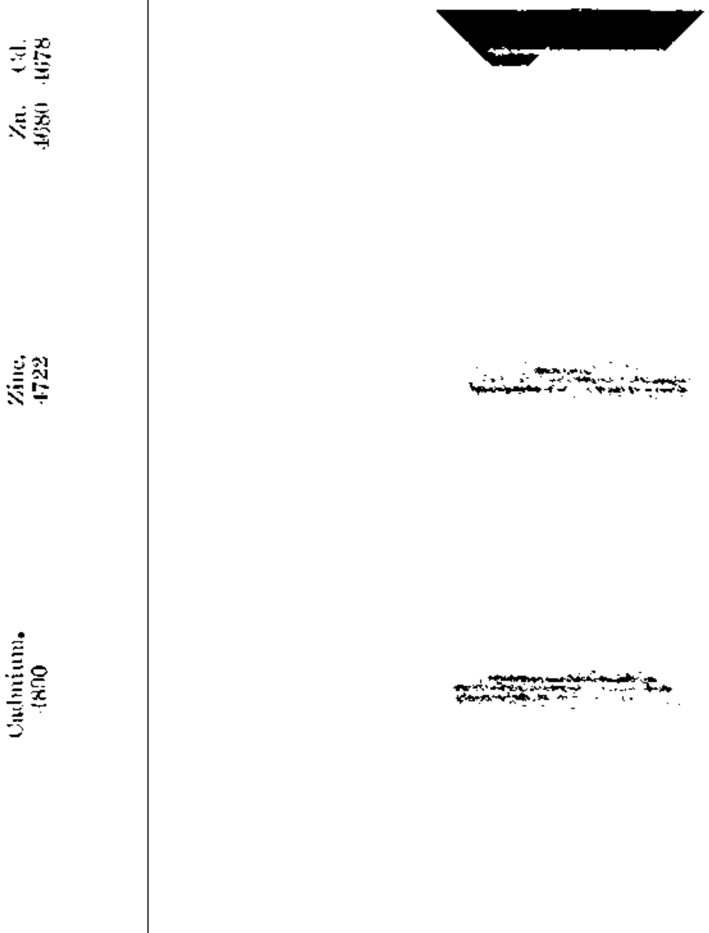

FIG. 36.—The top lines are not subject to the influence of the magnetic field. Underneath the same lines are shown affected by a magnetic field of great strength. The lines on the right are resolved into pure triplets, while those in the centre appear as quartets. From a photograph by Dr. Preston taken in 1897.

It was found that while *some* of the spectral lines were converted into triplets, *others* were resolved into quartets, sextets, octets, or other complex types, while others again remained almost if not altogether unchanged.

And then there was another revelation.

Not only do lines in the spectra of different substances vary in this respect, but lines in the spectrum of any one substance are differently changed; while some spectral lines of an element show a considerable resolution in the magnetic field, others are scarcely affected at all. This important fact was first stated by Dr. Preston in 1897.*

This brings us to the connection between this line of work and my own, for we now find lines *of the same substance* behaving differently *quâ* magnetic perturbations, as I found iron lines behaving differently in the spectra of sun spots *quâ* velocity. About this different behaviour *quâ* perturbation there is no question. I will refer to some of the work since done in this connection.

M. Cornu was the next to note the importance of this. He writes:—

"The effect of the magnetic field on the period of vibration of the radiations of the luminous source seems to depend, not only upon the chemical nature of the source, but also upon *the nature of the group of spectral lines to which each radiation belongs*, and on the part which it plays in this group."† Somewhat later MM. H. Becquerel and Deslandres gave details with regard to the spectrum of iron in the ultra-violet region, calling attention to these observations as being of great importance "physically, chemically, and astronomically."‡

Dr. Zeeman§ subsequently published the statement that observing across and along the lines of force, although the vast majority of iron lines were, with the field used, resolved into doublets, triplets, quadruplets, &c., *three or four lines seemed unaffected.* In the case of a few lines he further found inequality between the outer components of a triplet across, and of the corresponding doublet along, the lines of force.

Messrs. Ames, Earhart, and Reese next noticed further peculiarities about the behaviour of some of the iron lines.‖

When the radiation at right angles to the magnetic field was studied, each line in the spectrum was found in general to be broken up into three, the central component being plane polarized with its vibrations along the line of force, the two side components being plane polarized at right angles to these, their vibrations being at right angles to the field of force.

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* *Trans. Roy. Dub. Soc.*, vol. vi, p. 385 (1898), and vol. vii, p. 7 (1899).

† *Astrophysical Journal*, vol. vii, p. 163, 1898.

‡ *Comptes Rendus*, vol. cxxvi, p. 997; vol. cxxvii, p. 18.

§ *Proc. of Roy. Acad. of Sciences, Amsterdam*, June 25, 1898, and *Astrophysical Journal*, vol. ix, p. 47.

‖ *Astrophysical Journal*, vol. viii, p. 48.

Exceptions to this rule, however, were found in the lines having wave-lengths 3587·13, 3733·47, and 3865·67, which behaved in exactly the reverse manner. Two other lines at wave-lengths 3722·72 and 3872·64, were quadruplets, the central component, which had its vibrations along the line of force, being a close double. Some of the lines which showed no modifications whatever, were those at λλ 3746·06, 3767·34, 3850·12, and 3888·67.

These observers further noticed that the separation of the side components of the triplets seemed to be irregular; they found that there were certain lines in which the separation was nearly the same, but much greater than that of other lines where separations seemed to be quite closely alike. On this basis they separated the lines in the iron spectrum into two classes, in each of which the "magnetic separation" was the same, but in the one set much greater than in the other.

The lines belonging to these two sets were found to be practically identical with those sets of lines into which the iron spectrum breaks up when studied with reference to the shift produced by pressure, but this conclusion is not accepted by Dr. Preston.

I have already stated that on the simple theory we should get triplets only, as on the simple theory of thirty years ago we should have got motion of a solar vapour indicated by all the lines in a spectrum. The facts are equally against the simple theory in both cases.

The magneticians can now, however, by extending their theory, embrace and explain all the new, and at first sight extraordinary, phenomena. To show how they have done it, I cannot do better than quote from a lecture recently given by Dr. Preston, who is among the most successful investigators of this new branch of science.*

"According to the simple theory, every spectral line, when viewed across the lines of force, should become a triplet in the magnetic field, and the difference of the vibration frequency between the side lines of the triplet should be the same for all the spectral lines of a given substance. In other words, the precessional frequency should be the same for all the ionic orbits, or the difference of wave-length δλ between the lateral components of the magnetic triplet should vary inversely as the square of the wave-length of the spectral line under consideration. Now, when we examine this point by experiment, we find that this simple law is very far from being fulfilled. In fact, a very casual survey of the spectrum of any substance shows that the law does not hold even as a rough approximation; for, while some spectral lines show a considerable resolution in the magnetic field, other lines of nearly the same wave-length, in the same substance, are scarcely affected at all. This deviation is most interesting to those who concern themselves with the ultimate structure of matter, for it shows that the mechanism which produces the spectral lines of any given substance is not of the simplicity postulated in the elementary theory of this magnetic effect.

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* *Nature*, vol. 60, p. 178.

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"According to the prediction of the simple theory, the separation $\delta\lambda$ should be proportional to λ^2, and although this law is not at all obeyed if we take all the lines of the spectrum as a single group, yet we find that it is obeyed for the different groups if we divide the lines into a series of groups. In other words, if the lines of a given spectrum be arranged in a series of groups, the lines of the first group being denoted by the letters A_1, B_1, C_1, . . . , those of the second group by A_2, B_2, C_2, . . ., and so on, then the corresponding lines A_1, A_2, A_3, &c., have the same value for the quantity e/m, or, as we may say, they are produced by the motion of the same ion. The other corresponding lines, B_1, B_2, B_3, &c., have another common value for e/m, and are produced therefore by a different ion, and so on. We are thus led by this magnetic effect to arrange the lines of a given spectrum into natural groups, and from the nature of the effect we are led to suspect that the corresponding lines of these groups are produced by the same ion, and therefore that the atom of any given substance is really a complex consisting of several different ions, each of which gives rise to certain spectral lines, and these ions are associated to form an atom in some peculiar way which stamps the substance with its own peculiar properties."

The general law announced by Preston states the further remarkable fact that if we consider a group of chemically related metals such, for example, as magnesium, zinc, and cadmium, then the sets of lines into which the spectrum of any one of these may be divided as above, correspond set for set with those into which the lines of any other of these metals are divided, in such a way that the magnetic change of frequency (or e/m) for any one set is the *same* as that for the corresponding set in each of the other metals. This seems to point to the conclusion that the metals of the same chemical group are built up, in part at least, of ions which are the *same* in all the metals of the group.

It will be abundantly seen, then, that these new inquiries have presented exactly the same difficulties as the old ones, and that they have been met in exactly the same way, by establishing the fact that the spectra of elementary substances are not produced by the vibration of similar "atoms" or "ions," but by a series of different ones.

It is already pretty obvious that when ordinary spectroscopic observations, and the evidence supplied by "series," and these magnetic perturbations are completely correlated, we shall have taken a long step forward.

CHAP. XIII.—"FRACTIONATION" EVIDENCE.

IN the three previous chapters I have endeavoured to show that new methods of inquiry in the physical field all support the dissociation hypothesis. I have next to show that similar confirmation may be expected when the present ineffective chemical methods of analysis and determination are replaced by more stringent ones, such as those exemplified and foreshadowed by Sir William Crookes's patient fractionation work on yttria.

For the first definite chemical confirmation of my work I had to wait till 1883. In that year Sir William Crookes gave an account, in a Bakerian Lecture to the Royal Society, of his beautiful researches on yttria. In the lecture he gave a sketch of the train of reasoning by which he had been led to the opinion that systematic fractionation had split up this stable molecular group into its "constituents," and these were not yttrium and oxygen, as they should have been.

Subsequently in an address to the British Association at the Birmingham meeting in 1886, he gave an account of the method of fractionation which had led to these results.

The importance of the work on yttria in relation to the question of dissociation lies in the fact that by the variation in intensity of the various lines of the phosphorescence spectrum of yttria, Sir William Crookes was led to the view that more elements than one were in question—that the ordinary chemical processes had been quite unable to make anything but an element out of a mixture. As a result of his work he found five components "by a veritable splitting up of the yttrium molecule." This obviously strengthens the view that if our chemical resources were much greater than they are, the demonstration that other similar changes of intensity in the spectra of other elements would also be achieved.

I now quote Sir William Crookes on his method, which constitutes a veritable new engine of chemical research.

"Broadly speaking, the operation consists in fixing upon some chemical reaction in which there is the most likelihood of a difference in the behaviour of the elements under treatment, and performing it in an incomplete manner, so that only a certain fraction of the total bases present is separated: the object being to get part of the material in the insoluble, and the rest in the soluble, state. The operation must take place slowly, so as to allow the affinities—which, by the nature of the case, are almost equally balanced—time to have free play. Let us suppose that two earths are present, almost identical in chemical properties, but differing by an almost imperceptible variation in basicity. Add to the very dilute solution dilute ammonia in such amount that it can only precipitate half the bases present. The dilution must be such that a considerable time elapses before the liquid begins to show turbidity, and several hours will have to elapse before the full effect of the ammonia is complete. On filtering we have the earths divided into two parts, and we can easily imagine that now there is a slight difference in the basic value of the two portions of the earth, the portion in solution being, by an almost imperceptible amount more basic than that which the ammonia has precipitated. This minute difference is made to accumulate by a systematic process until it becomes perceptible by a chemical or physical test."

With reference to the result to which this most laborious research had led him, I will quote his own words,* remarking in the first instance that crude yttria from samarskite, gadolinite, cerite, and other similar minerals, is the raw material. The first operation is to free it roughly from earths of the cerium group, which is effected by taking advantage of the fact that the double sulphates of the potassium and the yttrium metals are easily soluble in saturated potassium sulphate solution, while the corresponding double sulphates of the cerium group of metals are difficultly soluble.

"No longer than twelve months ago the name yttria conveyed a perfectly definite meaning to all chemists. It meant the oxide of the elementary body, yttrium. I have in my possession specimens of yttria from M. de Marignac (considered by him to be purer than any chemist had hitherto obtained), from M. Clève (called by him 'purissimum'), from M. de Boisbaudran (a sample of which is described by this eminent chemist as 'scarcely soiled by traces of other earths'), and also many specimens prepared by myself at different times and purified up to the highest degree known at the time of preparation. Practically these earths are all the same thing, and up to a year ago every living chemist would have described them as identical, *i.e.*, as the oxide of the element yttrium. They are almost indistinguishable one from the other, both physically and chemically, and they give the phosphorescent spectra *in vacuo* with extraordinary brilliancy. This is what I formerly called yttria, and have more recently called *old* yttria. Now these constituents of old yttrium are not *impurities* in yttrium any more than praseodymium and neodymium (assuming them really to be elementary) would be impurities in didymium. They constitute a veritable splitting up of the yttrium molecule into its constituents."

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* *Chemical News* vol. liv, p. 1100.

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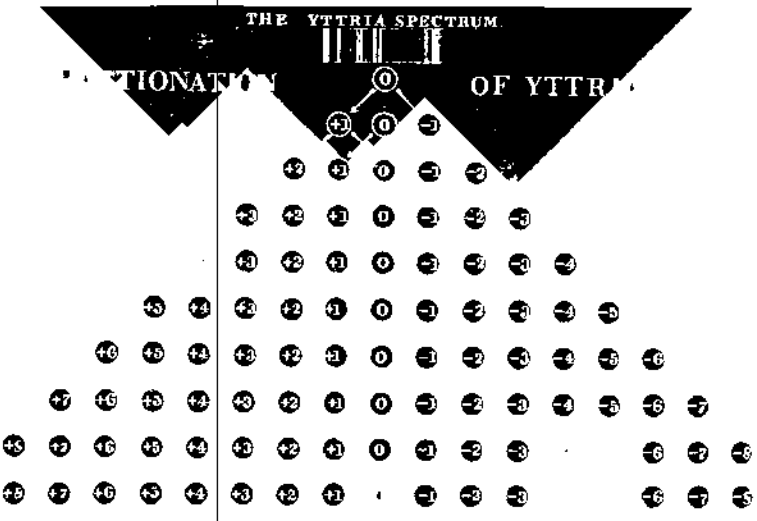

FIG. 37.—Showing how by the method of fractionation yttria is separated into five different substances, defined spectroscopically by the different intensities of the phosphorescent lines.

"The final result to which I have come is that there are certainly five, and probably eight, constituents into which yttrium may be split. Taking the constituents in order of approximate basicity (the chemical analogue of refrangibility) the lowest earthy constituent gives a deep blue band Gα (λ 482); then there is the strong citron band Gδ (λ 574), which has increased in sharpness till it deserves to be called a line; then come a close pair of greenish-blue lines, Gβ (λ 549 and λ 541, mean λ 545); then a red band, Gζ (λ 619), then a deep red band, Gη (γ 647); next a yellow band, Gε (λ 597); then another green band, Gγ (λ 564); this (in samarskite and cerite yttria) is followed by the orange line Sδ (λ 609). The samarium bands remain at the highest part of the series. These, I am satisfied, are also separable, although for the present I have scarcely touched them, having my hands fully occupied with the more easily resolvable earths. The yellow band, Gε, and green band, Gγ, may in fact be due to a splitting up of samarium."

So far as I know, Sir William Crookes has not yet named the elements differentiated by the lines of the wave-lengths to which he refers; but more recently, still dealing with yttria, he has made another research, having for its objective the separation of the element characterised by a group of lines in the neighbourhood of λ 3110. The discovery of victorium with an atomic weight near 117 has rewarded his efforts.

By following up the spectroscopic evidence, then, Sir William Crookes has already "split up" one "element" into five; another argument, if one were needed, that the spectrum of an element is produced not by similar but by dissimilar molecules.

BOOK IV.—OBJECTIONS TO THE DISSOCIATION HYPOTHESIS.

CHAP. XIV.—THE CHEMISTRY OF SPACE.

I HAVE now to refer to certain objections which have been urged against the views to which I have been giving expression during the last thirty years, views which seemed to me to indicate a way out of the tangle in which both laboratory, solar and stellar spectroscopic work have one after the other landed me, and as I have shown in the immediately preceding chapters, others after me, engaged in somewhat similar inquiries.

The objections to which it is of most importance to refer in this place have to do with the stellar evidence. I have supposed, and I think legitimately, until the contrary is proved—that is, the *onus probandi* lies with objectors—that the materials out of which, on the Meteoritic Hypothesis, worlds are eventually formed, are similar in all parts of space. Neither Kant nor Laplace thought of differentiating the ultimate chemistry of the material, and indeed the only view of special differences which has been put forward to my knowledge in recent years was a subtle one suggested by a learned divine to account for miracles. On this theory, in certain parts of space miracles might happen, in others not; and the movement of the solar system through space provided us with the necessary changes of this condition.

But quite recently this view has been extended to the chemical conditioning of space, and the first most general objection I have to meet is that the various spectral differences which it has been my duty to chronicle as defining the various groups of stars, are not brought about by temperature, but are due simply to the fact that the chemistry of space varies, so that in consequence of their locus of origin and their environment, some stars may be composed chiefly of the cleveite gases, others of hydrogen, others of calcium, others of iron, others of carbon, and so on.

But it is assumed that there may be some cases, not so extreme as these, so that only the *relative* composition may vary from star to star. This view of space divided into chemical parishes is supposed to be supported by the alleged localisation of stars of the same type in particular parts of space (as indicated by proper motions, &c.).

A possible *vera causa* of such chemical differentiation was, I believe, suggested by Dr. Wolf, who in 1866,* misled by Sir Wm. Huggins' statements concerning the chemistry of the nebulæ, endeavoured to explain their spectra, and therefore their chemical constitution as distinguished from that of stars, in a way that will be gathered from the following extract:—

"If we admit the data of spectrum analysis as to the gaseous state of these singular bodies (the nebulæ), and the simplicity of their composition, one is led to see in them only the residuum of the primitive matter after condensation into suns and into planets has extracted the greater parts of the simple elements which we find on the earth, and chemically in some of the stars."

It will be seen that Wolf considers only the differentiation of nebulæ from suns by the "extraction of matter" by some previous local action. The chemistry was general to begin with, then the residuum was worked up.

Dr. Schuster, however, has more recently gone further, still starting however from a general chemistry:—

"We have no reason to believe that the nebulæ of the present day resemble our sun's ancestor. Some of the stars which are now in an early stage of development, may be forming through the condensation of matter which has been left over by others; and it would not be surprising if the youngest star did not agree in constitution with its aged companions."†

Let us suppose then that the number of different chemical parishes in space is legion to begin with, and that by such actions as those suggested by Drs. Wolf and Schuster more differences are established, surely the stellar differences must be legion too. I would submit that the more such causes as these be added to a hypothetic irregular distribution of different kind of matter in space, the more differences in the chemical constitution of stars should be found. But this is not so according to the facts.

While the number of chemical elements known at the present time is over seventy, the number of well-marked groups of stars is only ten, if we take one side of the temperature curve; that is, if we deal with stars increasing or stars decreasing their temperature. We are justified in using one side only, because the spectra of stars on opposite sides of the temperature curve indicate precisely the same elements, though the percentage composition of *effective absorbing* regions is different in the two cases. At the same temperature on opposite sides, the chief difference is in the inversion of the intensities of the hydrogen and the metallic lines.

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* *Hypothèses Cosmogoniques*, p. 7.

† *Proc. Roy. Soc.*, vol. 61, p. 209.

Hence the facts are distinctly against the view of different chemical parishes in space; they also suggest that we are not justified in even conceding possible variations in the percentage composition. On this ground an infinite variety of spectra might be expected, but, as already stated, the number of well-marked groups is ten.

The Sun, Capella, and Arcturus, and other cooling stars, enormously separated in space, contain the same spectral lines with almost identical intensities, so that not only do they contain the same "elements," but they contain them *in absolutely identical proportions*. The earlier and hotter stages of such stars could not therefore have consisted of different mixtures.

Again, all the blood-red stars, which it is generally acknowledged are near the point of extinction, have practically identical spectra.

Another strong argument against the objection now under discussion is that each particular kind of star spectrum is always associated with the same degree of stellar temperature as determined by other considerations, chiefly the extension of the spectrum into the ultra-violet. With differences of chemical composition, different spectra would occur with equal temperatures.

We are therefore justified in the conclusion that the differences recorded in stellar spectra do not come from a different percentage composition of the elements present, but arise from the action of different temperatures in the same molecules; and until the above facts are explained, I must hold that the argument is complete that we do get the same elements represented by different spectral lines in different stars when the apparent differences are such as to suggest the objection to which I am now referring. It is not a question of the absence of elements, but of the *absence of certain molecular complexities* of each element, which separates the spectrum of the sun from those of the stars of various orders.

Having said so much regarding the objection generally, I must now proceed to discuss the only piece of evidence which has been brought forward in support of it, namely, the alleged localisation of certain chemical groups of stars in particular parts of space, arising from the fact that certain of the chemical elements are only to be found in certain regions. This localisation is not held to be a quantitative one merely, that is, depending upon varying proportions of elements, but upon their absolute absence here and there.

I propose to discuss this question in the following way.

Since we can only deal with the masses of matter in space which are visible, it is obvious that any inquiry into the distribution of the chemical conditionings, as revealed by spectra, of these masses must be preceded by an inquiry into the distribution of the visible masses, considered merely as masses, and quite independent of chemistry.

We must therefore first deal with the general distribution of the stars and nebulæ, independently of their chemistry. That will give us a general idea of our stellar system.

Having this as a basis, we can next see whether stars of the same chemistry are seen along the same radius (taking our solar system as the centre) or the same direction in space. Next, taking distances into account, we can see if there be any proof of different chemical shells, so to speak.

It must be borne in mind that *a greater or less proportion* of stars of the same chemical quality in certain regions will not touch the question. We can only deal with demonstrations of the absence of certain chemical elements in certain regions, so far as the stars supply us with evidence.

Chap. XV.—The General Distribution of Stars.

The labours of three or four generations of astronomers have conclusively proved that the distribution of the stars well within our ken is dominated by the Milky Way. Although the Milky Way to the naked eye looks very unlike the other parts of the heavens, we have known since the time of Galileo that the difference arises from the fact that it is composed of a tremendous multitude of stars, a very large percentage of the masses of matter which compose our system lying in its plane; it does not merely represent a fiery or igneous fluid, as different schools thought it did in the olden days. A small opera-glass or telescope easily shows us that we are in presence of an innumerable multitude of stars.

The Milky Way is a great circle inclined at an angle of about 62 to the earth's equator or to the equatorial plane extending to the stars. We know nothing, of course, of the reason for that angle of 62°, but it has its importance, because not only must the belt cross the equator at two opposite points, as it does in two opposite constellations, Aquila and Monoceros, but the poles of the Milky Way must lie at the points of greatest distance from the junction with the equator in certain constellations. These are Coma Berenices and Sculptor, and the position of the N. galactic pole, as the north pole of the Milky Way is called, is in R.A. 12 h. 40 m. Dec. + 28°.

When we come to look at the Milky Way a little more closely, we find that from two points in it branches are thrown out, so that over some part of its orbit, so to speak, it is double. The great rift which separates these two parts of it begins near a star in the southern hemisphere, α Centauri, and it continues for more than six hours in right ascension until the two branches meet again in the constellation Cygnus, which is well within our ken in the northern heavens. The distance apart of the middle lines of these two components of the Milky Way, where the split is most obvious, is something like 17°, so that, in addition to the angle of 62° from the ecliptic, in some part of the Milky Way, there is another offshoot springing out of it at an angle of something like 17°. The regions of greater brilliancy correspond approximately to the places where the branches intersect each other. In short, there are sundry indications that the whole phenomena of the Milky Way may become simplified by treating it as the resultant of two superimposed galaxies. The general view till recently was that the Milky Way is not a great circle, because it was thought the sun was not situated in its plane. The whole mass of stars was likened to a millstone split along one edge, which was Sir William Herschel's first idea. But the recent work, chiefly of Gould in Argentina, has shown that it practically is a great circle. However that may be, in one part of the heavens this wonderful Milky Way appears as a single, very irregular, stream, and in another part it appears to be duplicated.

This galaxy of stars is full of wonderful majesty and complexity. We find in it indications of delicate markings going out into space, apparently coming back strengthened; of streams in all directions; of clusters clinging to those streams, and so on. In other parts it is curdled, which is the only term which I can use to express my meaning. In one region we may find it absolutely free from any important stars; in another we may find it mixed with obvious nebula; and in another we may find it mixed not only with obvious nebula, but with a great number of bright-line stars involved not only in the Milky Way, but in the nebula itself.

We have now, fortunately for science, priceless photographs of these different regions which give us an idea of the enormous number of stars in some parts, and of the streams of nebulous matter which are seen in the Milky Way from region to region. Here we find a regular river of nebulous matter rushing among thousands of stars, elsewhere the galaxy seems to tie itself in knots. There is an individuality in almost every part of it, which we can study on our photographic plates; practically there are no two parts alike. Other photographs bring before us the curdled appearance which is visible in different regions, and finally the connection of the infinite number of stars with obvious nebulous matter. In this way, then, we are enabled to form an idea of the general conditioning of things as we approach the Milky Way.

The next important point is that the enormous increase of stars in the Milky Way is not limited to the plane itself, but that there is really a gradual increase from the poles of the Milky Way, where we find the smallest number of stars. It is not very easy to bring together all the information, for the reason that different observers give different measures; they take different units for the space they have determined to be occupied by stars from the pole towards the galactic plane; and also the number of stars in the northern hemisphere is not the same as the number in the southern hemisphere. But roughly speaking we may say, if we represent the number of stars at the galactic pole by four, the number of stars in the galactic plane will be about fifty-four.

The following table will show the gradual increase in the number of stars from the pole to the plane, as seen by the Herschels with a reflecting telescope of 18 inches aperture and 20 feet focal length :—*

Galactic polar distance.	Average number of stars per field of 15′.	
	Northern.	Southern.
0°–15°	4·32	6·05
15–30	5·42	6·62
30–45	8·21	9·08
45–60	13·61	13·49
60–75	24·09	26·29
75–90	53·43	59·06

A consideration of the distribution of stars in right ascension between declinations 15° N. and 15° S., led Struve to the conclusion that there are well marked maxima in R.A. 6 hrs. 40 mins. and 18 hrs. 40 mins., and minima in R.A. 1 hr. 30 mins. and 13 hrs. 30 mins.; he remarks that the maxima fall exactly on the position of the Milky Way in the equator, and further states that "the appearance of the close assemblage of stars or condensation is closely connected with the nature of the Milky Way, or that this condensation, and the appearance of the Milky Way, are identical phenomena."

Although the Milky Way dominates the distribution of stars, and especially of the fainter stars, it does not appear to be the only ring of stars with which we have to do. Sir John Herschel traced a zone of bright stars in the southern hemisphere, which he thought to be the projection of a subordinate shoot or stratum. That was the first glimpse of a new discovery, which was subsequently established by Dr. Gould in his work in the southern hemisphere at Cordova. He found that there was a stream of bright stars to be traced through the entire circuit of the heavens, forming a great circle as well defined as that of the galaxy itself, which it crossed at an angle of about 25°.

Gould, while in the southern hemisphere, had no difficulty in observing that along this circle, which we may call the Star Way, in opposition to the Milky Way, most of the brighter stars in the southern heavens lie.

When he subsequently came home he made it a point of study to see whether he could continue this line of bright stars completely through the northern hemisphere, and he found no difficulty. So that we may now say that the existence of this supplementary Star Way, indicated by the line of extremely bright stars, is beyond all question.

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* *Outlines of Astronomy*, Herschel, pp. 535, 536.

I quote the following from what Gould has written on this subject:—*

"Few celestial phenomena are more palpable there than the existence of a stream or belt of bright stars, including *Canopus*, *Sirius*, and *Aldebaran*, together with the most brilliant ones in *Carina*, *Puppis*, *Columba*, *Canis Major*, *Orion*, &c., and skirting the Milky Way on its preceding side. When the opposite half of the galaxy came into view, it was almost equally manifest that the same is true there also, the bright stars likewise fringing it on the preceding side, and forming a stream which, diverging from the Milky Way at the stars α and β *Centauri*, comprises the constellation *Lupus*, and a great part of *Scorpio*, and extends onwards through *Ophiuchus* towards *Lyra*. Thus a great circle or zone of bright stars seems to gird the sky intersecting with the Milky Way at the Southern Cross, and manifest at all seasons, although far more conspicuous upon the *Orion* side than on the other. Upon my return to the North, I sought immediately for the northern place of intersection; and although the phenomenon is by far less clearly perceptible in this hemisphere, I found no difficulty in recognising the node in the constellation *Cassiopeia*, which is diametrically opposite to *Crux*. Indeed it is easy to fix the right ascension of the northern node at about 0 hr. 50 mins., and that of the southern one at 12 hrs. 50 mins.; the declination in each case about 60°; so that these nodes are very close to the points at which the Milky Way approaches most nearly to the poles. The inclination of this stream to the Milky Way is about 25°, the Pleiades occupying a position midway between the nodes."

Gould also had no difficulty in showing that the group of the fixed stars to which I have just referred, at all events of fixed stars brighter than the fourth magnitude, is more symmetrical in relation to this new star line than to the Milky Way itself, and that the abundance of bright stars in any region of the sky is greater as the distance from this new star line becomes less. Practically 500 of the brightest stars can be brought together into a cluster, independent of the Milky Way altogether—a cluster he points out of somewhat flattened and bifid form.

Connection of the Milky Way with Nebulæ.

Not only do we find that the stars are very much more numerous near the Milky Way than elsewhere, but that the same thing happens with regard to the planetary nebulæ. Nebulæ generally we cannot at present discuss with any advantage, because there are very many bodies classed as nebulæ in the different catalogues about the physical natures of which we know absolutely nothing. I shall only call attention to those points about which we can be most certain.

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* *Amer. Jour. Sci.*, vol. viii, p. 332.

Not only do we find stars and planetary nebulæ increasing in number as the Milky Way is approached, but the undoubted star clusters also increase towards the Milky Way in a marvellous manner.

Bauschinger[‡] (1889) in a review of Dr. Dreyer's "New General Catalogue" (7,840 objects), discussed the distribution of different classes of objects and found that star clusters, by which he means of course resolved clusters, and planetary nebulæ congregate in and near the galaxy.

Mr. Sydney Waters some four years later, in 1893, brought together the nebulæ and the star clusters on maps which showed, in a most unmistakable manner, that the star clusters, like the planetary nebulæ and stars generally, are very much more numerous in the plane of the Milky Way than they are in any other part of the heavens.

It is striking to note the fidelity with which the clusters follow not only the main track of the Milky Way, but also its convolutions and streams, while the remarkable avoidance of the galaxy by the nebulæ, excluding the planetary nebulæ, is obvious; it was indeed noted by Sir Wm. Herschel.

We have seen, then, that the greatest number of stars congregate in the plane of the Milky Way, and the greatest number of planetary nebulæ and the greatest number of star clusters.

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[*] *V. J. S. Ast. Ges.*, vol. xxiv, p. 43.

CHAP. XVI.—THE DISTRIBUTION OF CHEMICAL GROUPS OF STARS.

A. *In Relation to Direction.*

THE most convenient way to consider the distribution of the various chemical groups of stars, is to take the plane of the Milky Way as a base, as we have already done regarding the stars merely as masses of matter independently of all chemistry, and to note whether any particular chemical species of stars congregates in the Milky Way or avoids it. In this way the new molecular inquiry will be on all fours with the older molar one.

I will begin by leaving distances out of consideration.

At present it will be sufficient for our purpose to deal with the more generalised classification (already given on p. 72), which is as follows :—

Highest Temperature.
Gaseous stars { Proto-hydrogen stars.
Gaseous stars { Cleveite-gas stars.
Proto-metallic stars.
Metallic stars.
Stars with fluted spectra.
Lowest Temperature.

In discussing the work of other observers I have, as far as possible, transposed the different notations employed into the chemical one given above, and in some cases the two arms of the temperature curve will require to be considered.

The first attempt at such an inquiry as this was made in 1884, by Dunér,* who had made himself famous by his admirable observations on two different classes of stars—those which I have referred to as being defined by carbon flutings in one case, and metallic flutings in the other. His work was practically the only research on the carbon stars—the stars, that is, with carbon flutings. He was, naturally, anxious to see how they were distributed, and he gave the number of these stars in varying parts of the heavens in relation to the Milky Way. He found that the numbers increased towards the Milky Way. The table I give will show the general result at which he arrived.

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* *Étoiles de la troisième Classe*, p. 126.

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We saw in the case of the ordinary stars that a very rapid progression in number is to be noticed from the pole of the Milky Way to the plane; we had three stars at the pole when we had fifty-three in the plane.

Distance from galactic pole.	Number.	Mean magnitude.
0°–35°	3	6·6
35 –60	8	6·6
60 –70	8	7·2
70 –80	13	7·4
80 –90	29	8·3

Dunér found, with regard to his carbon stars, that there was distinctly an increase from the pole towards the plane, but we observe that the rate of increase is very much less in this case; so that, starting with three at the pole, he only found twenty-nine in the plane. Although then it was true that the number of stars did increase towards the Milky Way, they did not increase so rapidly as the stars taken as a whole; still, from his observations, we are justified in stating that there is an increase as we approach the plane of the Milky Way. They are, therefore, not limited to the plane.

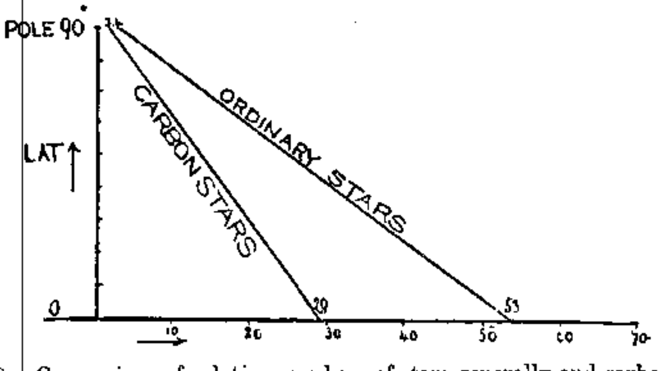

FIG. 38.—Comparison of relative numbers of stars generally and carbon stars.

That was in 1884. In 1891 Professor Pickering, when he found that he had collected something like 10,000 stars in the Draper catalogue, began to consider their distribution in different parts of space in relation to the then classification, which was practically one founded on hieroglyphics, since we knew very little about the chemistry of the different bodies at that time.

He found that the Milky Way was due to an aggregation of white stars, by which he meant, as we now know, very hot stars, and the hottest of them, that is the gaseous ones, exist more obviously in the Milky Way than do the others. The proportional number of proto-metallic stars in the Milky Way was greater for the fainter stars than for the brighter ones of this kind, and that at once suggests a possibility that in the Milky Way itself there is a something which absorbs light; so that the brightest stars are apt not to be really the brightest, but apparently bright because they have not suffered this absorption, and that those which have suffered this absorption may be very much further away from us than the others of a similar chemistry. He also arrived at this extremely important conclusion, namely, that the metallic stars, that is, stars like our sun, stars more or less in their old age, had no preference for the Milky Way at all, but are equally distributed all over the sky. With regard to the group of stars known by metallic flutings in their spectra, he has no information to give us any more than Dunér had, for the reason that their number is small, and they have not yet been completely studied.

Only last year this inquiry was carried a stage further by Mr. McClean, who not only photographed a considerable number of stellar spectra in the northern hemisphere, but subsequently went to the Cape of Good Hope in order to complete the story with reference to the stars down to the third or fourth magnitude which he could observe there. He was very careful to discuss, in relation to the Milky Way and certain galactic zones, the distribution of the various kinds of stars which he was fortunate enough to photograph.

He found that if we deal with the gaseous stars the numbers in the north and south polar region are small, and that the numbers nearer the Milky Way are greater, so that finally we can see exactly how these bodies are distributed. If we take the gaseous, that is to say the hottest stars, we find the smallest number in the polar regions; but if we take the metallic stars we find practically the largest number, at all events a considerable number, in the polar regions. The general result, therefore, is that the gaseous stars are mostly confined to the galactic zones, the proto-metallic stars, that is those down to about 3½ magnitude, are not so confined. What is also shown is that the metallic fluting stars are practically equally distributed over the polar regions and over the plane of the Milky Way itself; so that, in that respect, we get for these stars very much the equivalent of the result arrived at by Dunér for the carbon stars, that is to say, they have little preference for the Milky Way.

Bright-line Stars.

These, then, are the results with regard to the stars having obviously dark lines in their spectra, but besides these there are many so-called bright-line stars.

I should say that there has necessarily been a change of front in our views with regard to these bright-line stars since they were first classified with nebulæ. The nebulæ are separated generically from the stars by the fact that in their case we have to deal with bright lines, that is to say, we deal only with radiation phenomena, and not with absorption phenomena, as in the case of the stars so far considered; and in the first instance it was imagined that the bright-line stars were, from the chemical point of view, practically nebulæ, although they appeared as stars, because the brightest condensations of them were so limited or so far away that they gave a star-like appearance in the telescope.

Since that first grouping of bright-line stars, by the work chiefly of the American astronomers, it has been found that in a large number of cases *they have also dark lines in their spectra*, and that being so we must classify them by their dark lines instead of by their bright ones; and the bright-line stars thus considered chiefly turn out to be gaseous stars, *with a difference*. What is that difference? It is this, I think: in the case of the bright-line stars we are dealing with the condensations of the most disturbed nebulæ in the heavens, together with the light which we get from the nucleus of that nebula which appears as a star, and can be spectroscopically classified with the other dark-line stars, inasmuch as the surrounding vapours close to the star produce absorption, and therefore give us dark lines; other parts of the nebulæ, probably those further afield, give us bright lines which mix with the dark ones. Therefore we get both bright lines and dark lines under these conditions. So far as the result goes up to the present moment, it looks as if we have now to consider that these bright-line stars, instead of being nebulæ merely, are gaseous stars at a very high temperature, in consequence of the fact that the nebula which is surrounding them, which is falling upon them, is increasing the temperature of the central mass by the change of *vis viva* into heat. Pickering,* in his discussion of these stars, had thirty-three to deal with, and he found that there was a wonderful tendency among these to group themselves along the Milky Way: that very few of them, in fact, lay outside its central plane; the galactic latitude, the distance in degrees from the plane being limited in the generality to only 2°, and the greatest departure, the greatest galactic latitude, was something within 9°. That was the story in 1891. Two years afterwards Campbell, another distinguished

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* *Astr. Nach.*, No. 2025.

American astronomer, also interested himself in this question of the bright-line stars, and he discussed them, his catalogue containing fifty-five as opposed to Pickering's thirty-three. He found also that they were

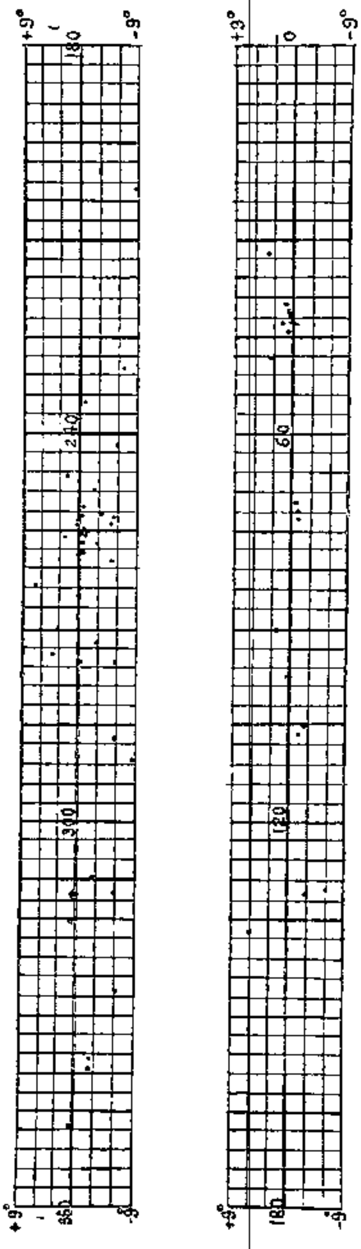

FIG. 39.—Distribution of the bright-line stars in the Milky Way.

collected almost exclusively in the Milky Way, and that outside the Milky Way practically none had ever been observed. The importance of this result I will indicate by and by. The central line of the map

(Fig. 39) represents the galactic zone, the plane of the Milky Way, and along it the different galactic longitudes are indicated, above and below the plane a few degrees of galactic latitude north and south are shown, sufficient to enable all the bright-line stars which Campbell discussed to be plotted. The map shows that all the bright-line stars

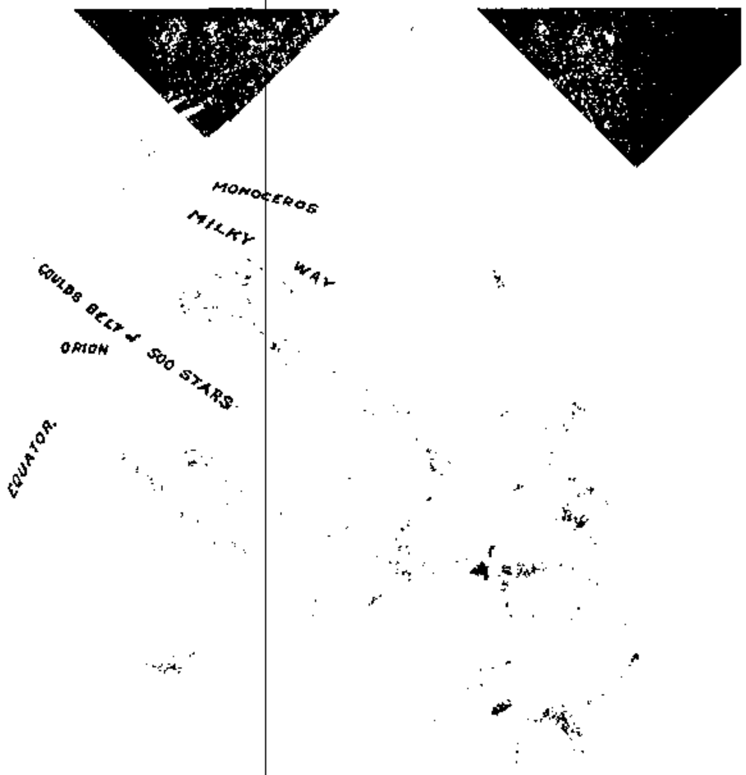

FIG. 40.—Photograph of a glass globe showing the relation of the Milky Way to the Equator and to Gould's belt of stars.

really are close to the central plane of the Milky Way. Only one out of the fifty-five is more than 9° from it, and this lies in a projecting spur, so that we cannot really say that that is out of the Milky Way.

It is remarkable that these bright-line stars are not equally distributed along the Milky Way. They are chiefly condensed in two opposite regions, and there is one region in which they are markedly absent.

Figs. 40 and 41 are photographs of a glass globe, on which are indicated the Milky Way; the secondary Milky Way, which starts from it at one point of the heavens and meets it again, is also shown; together with Gould's Star Way and the equatorial plane. The dark wafers indicate the positions of the bright-line stars.

Fig. 41.—The Milky Way, where double in relation to the Equator and Gould's belt of stars, showing that the bright-line stars (dark wafers) and new stars (white wafers) are limited to the Milky Way.

We find that these stars begin just before the doubling commences. They continue along the plane, and are sometimes very numerous, and they end just after the doubling ends; and we notice there is a long range of the Milky Way where it is single in which there is absolutely no bright-line star at all. It looks, therefore, very much as if there is a something connected with this doubling of the Milky Way which produces the conditions which generate these bright-line stars.

By the labours of Dunér, Pickering, McClean and Campbell, we are beginning to get very definite notions as to the distribution of the various chemically different stars in relation to the Milky Way. As I have already noticed, there can be no question as to the intimate association of the bright-line stars with nebulæ. We must next then consider the nebulæ from the point of view of chemical distribution, but here we are somewhat in a difficulty.

I have already stated that with regard to the general question of the nebulæ it is impossible to speak with certainty, because at present there has not been sufficient time and there has not been a sufficient number of observers at work to classify the thousands of "nebulæ" which we now know of into those which give us the gaseous spectrum and those which are entirely different, apparently, in their constitution, and only give us what is called a continuous spectrum. Still we can go a little way in this direction by means of some figures which I have noted. The point is to see whether there is any difference in the distribution of those nebulæ which are undoubtedly masses of gas, which give us the so-called nebulous spectrum, and those other nebulæ about which at present we know very little, which give us so-called continuous spectra. It is clear that on this point undoubtedly, at some future time, a great deal will be learned. The figures I give bring the results up to the year 1894. If we take the region near the Milky Way, the region bounded by 10° galactic latitude north and south, and consider the planetary nebulæ, we find that there are forty-two; but if we deal with those which are further than 10° from the Milky Way, that number drops to five. If we take other nebulæ, not necessarily planetary but gaseous like planetary nebulæ, inasmuch as they give us a spectrum of bright lines, we find that there are twenty-two in or near the Milky Way, and only six outside. If we take the so-called nebulæ known to have continuous spectra, which need not be nebulæ at all—we only imagine them to be nebulæ because they are so far away that we cannot get a really true account of them—we find that the conditions are absolutely reversed. There are only fourteen of them in the plane of the Milky Way, but there are forty-three lying outside it; so that the percentage within 10° of the Milky Way comes out to be eighty-four in the case of the planetary and the other nebulæ which give us bright lines, and in nebulæ with continuous spectra only twenty-five. Therefore we get an absolute identity of result with regard to the bright-line stars and the other objects which give us bright-line spectra.

There is another class of bodies of extreme interest. In fact, to some they are more interesting than all the other stars in the heavens, because they are the mysterious "new stars," which have been supposed to be new creations. When we come to examine these so-called new stars we find that they also are almost absolutely limited to the Milky Way. Our information begins 134 years before Christ, and it ends last year. The number of stars thus reported as new stars is thirty-one, and of these only three have been seen outside the Milky Way. Fig. 40 shows what the facts are with regard to the new stars. The bright-line stars being distinguished by dark wafers, the new stars are shown by white wafers. We notice that where we get practically the greatest number of dark wafers we get a considerable number of white ones. That means that these new stars take their origin in the same part of space as that occupied by the bright-line stars, and it is also interesting to point out that the void indicated where the Milky Way is single, where there were no bright-line stars, is equally true for the new stars; only one new star has been recorded in this region (Fig. 41).

As I have said, a great deal of interest has been attached by many people to the question of the new stars, for the reason that whenever a new star appeared in a part of the heavens where no star was seen before, it was imagined that something miraculous and wonderful had happened. That was justifiable while we were ignorant, but recent work has shown, I think almost to a certainty, that the real genesis of a new star is simply this. We have near the Milky Way a great number of nebulæ, planetary or otherwise; we have more planetary nebulæ near the Milky Way than in any other part of the heavens; the nebulous patches also observed in it may include streams of meteorites rushing about under the influence of gravity; the origin of a new star is due to the circumstance that one of these unchronicled nebulæ suddenly finds itself invaded by one of these streams of meteorites. There is a clash. These meteorites we know enter our own atmosphere at the rate of thirty-three miles a second, and we may therefore be justified in assuming that any meteoritic stream in space, even in the Milky Way, would not be going very much more slowly. If we get this rapidly-moving stream passing through a nebula, which is supposed to be a mass of meteorites more or less at rest, of course we must get collisions; of course, also, we shall get heat, and therefore light. When the stream has passed through the nebula the luminosity will dim and ultimately, attention having been called by this cataclysm to that particular part of space, we shall find that there is a nebula there. This has always been so; and therefore in the case of new stars we must always expect to get indications of the existence of two bodies, the intruder and the body intruded upon.

We must also expect, if we are dealing with small particles of meteoritic dust, that the action will be very quick, and that the war will be soon over. All this really agrees with the facts. In the case of the new star we were fortunate enough to have the opportunity of observing in the northern hemisphere, not very long ago, the new star in the constellation Auriga, we obtained undoubted indications of the fact that we were dealing with two different masses of matter; for the reason that if we take the chief spectral lines marked G, h, H and K (Fig. 42), that is to say, the lines of hydrogen and of calcium, we find both bright lines and dark lines, which being interpreted means that hydrogen and calcium were both giving out light and stopping light. We cannot imagine that the *same* particles of calcium and of hydrogen were both giving out light and stopping light; there must have been some particles of hydrogen and calcium giving light and others stopping light; and if we look at the photograph carefully we find that the bright lines and the dark lines are side by side, and we know that that means a change of wave-length in consequence of movement, and we also know from the change of wave-length indicated that the differential velocity of the particles which gave us the bright hydrogen and calcium, and the dark hydrogen and calcium, must have been something like 500 miles a second. In that way we obtained indisputable proof that we were really dealing with two perfectly different series of particles moving in opposite directions, and that that was the reason we got that sudden illumination in the heavens which as suddenly died out until finally a nebula previously undiscovered was found to occupy the place. The nebula is really not the result, the nebula was the cause, but we did not know of its existence until our special attention had been drawn to that part of the heavens.

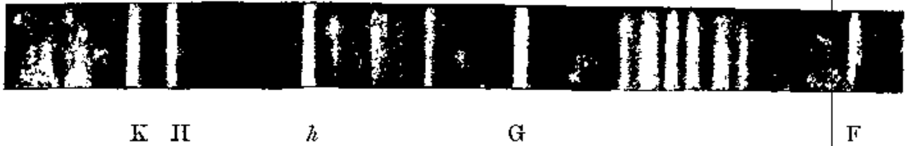

FIG. 42.—The spectrum of Nova Aurigæ, showing both bright and dark lines.

B. *In relation to Distance.*

So much, then, for the first statement of facts relating to the distribution of the various star groups and nebular groups in the most general form. The next question is, can we say anything about the distances of these bodies?

The way in which an astronomer attempts to determine the different distances of the various stars from the earth, may be very well grasped by considering what happens to any one travelling in a railway train. If the train be going fairly quickly, and we look at the near objects, we find that they appear to rush by so rapidly that they tire the eye; the more distant the object we look at is the more slowly it appears to move, and the less the eye is fatigued. Now, suppose that instead of the train rushing through the country and passing the objects which we regard under these different conditions, the different objects are rushing past us at rest. Then, obviously, those things which appear to be moving most quickly will be those nearest, and the more distant objects, just because they are distant, will appear to move more slowly; that is to say, we shall get what is called a large "proper motion" in the case of the objects nearest to us, and a small "proper motion" in the case of the bodies which are further away.

This question has been attacked with regard to the stars in magnificent fashion by a great number of astronomers.

It was Mr. Monck who was the first to show in 1892* that the gaseous stars had the smallest proper motion; that is to say, that the hottest stars were further away from us than the cooler ones. He next found that the proto-metallic stars—that is to say, the stars not so hot as the gaseous, but hotter than the metallic stars—had the next smaller proper motion. This, of course, indicates that the metallic stars are the nearest to us unless proper motion does not depend upon distance, but rather upon a greater average velocity in space. It has been shown, however, by considering the sun's movement in space, that this view probably may be neglected. The first discussion of proper motion, then, went to show, roughly, that the hotter a star is the further away from us it is; and it made out a fair case for the conclusion that the sun forms one of a group or cluster of stars in which the predominating type of spectrum is similar to its own.

Kapteyn carried the inquiry a stage further.† Working upon the idea that stars with the greatest proper motion are on the average the nearest, the part of the proper motion due to the sun's translation in space he considered must depend strictly upon the distance, and he determined this by resolving the observed proper motion along a great circle passing through the point of space towards which the sun is moving, which is called the apex of the sun's way, and reducing to a point 90° from the apex. His results were practically the same as those obtained by taking the individual proper motions. He also found that stars with the greatest proper motion are mainly metallic, and have no regard at all to the Milky Way; that stars with the smallest and no observable proper motion are gaseous and proto-metallic, including a few metallic ones which have collected in the galactic plane. In this he agrees with the prior observations to which I have drawn attention. In the table which I now give the mean proper motion is shown.

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* *Astronomy and Astro-Physics*, vol. xviii, 2, p. 876.

† Amsterdam Academy of Science, 1893.

Relation between Spectra and Proper Motions of Stars (Kapteyn).

Mean proper motion.	Gaseous and proto-metallic stars.	Metallic stars.	Metallic flutings.	Ratio, metallic to gaseous.
1″·39	3	51	—	17·0
0·52	12	66	1	5·5
0·35	14	66	—	4·7
0·24	34	124	—	3·6
0·18	35	67	3	1·9
Inappreciable	79	35	1	0·44

We find that the gaseous and proto-metallic stars increase in number as the proper motion decreases. We find also the ratio of the metallic to the gaseous and the proto-metallic. We begin with a ratio of 17, and end with something like a ratio of 0·4; so that the results may be considered to be pretty definite. These results were obtained by Kapteyn with 591 stars which were common to Stumpe's catalogue of proper motions and the Draper catalogue dealing with spectra. The general result may, therefore, be stated that at the nearest distance the metallic stars are seventeen times more numerous than gaseous stars, and at the greatest distance they are not half the number.

Here again the question arises, how far the intrinsic brightness of these bodies, in relation to their distance from us and the possible greater or less extinction of light in space, has to be taken into consideration. That is a problem which will require a considerable amount of work in the future. It is rather remarkable that if we take the stars with very great proper motion, very much greater than the average, we find with regard to four that three of them are undoubtedly metallic, but it is possible that the star 1830 Groombridge, which is always looked upon as the star which beats the record in velocity seeing that it would travel from London to Pekin in about two minutes, is not a metallic star.*

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* These stars are—

1830 Groombridge	7″·04 ..	Gaseous or proto-metallic.
Σ 2758	5·196 ..	Metallic.
Σ 578	4·049 ..	Probably metallic.
D.C. 583	3·7 ..	Metallic.

CHAP. XVII.—THE RESULT OF THE INQUIRY.

WE are finally in a position to make a general summary of the distribution of the various chemical groups of stars not only in relation to their direction in space, as seen from the solar system, a direction most conveniently considered in relation to galactic latitudes and longitudes, but also in relation to their distance from us.

The results arrived at in the two previous chapters may be summarized as follows. First we will consider the stars studied by their absorption phenomena.

Group.	Relation to Milky Way.	Proper motion.
Gaseous stars ..	Condensed in Milky Way (Pickering and McClean)	Smallest* (Monck).
Proto-metallic ..	Brighter ones not notably condensed in Milky Way (McClean) Tend to collect in Milky Way more especially the fainter stars (Pickering)	Intermediate (Monck).
Metallic	Not condensed in Milky Way (Pickering and McClean)	Div. 1. Greatest (Kapteyn).
	Collected in Milky Way (Kapteyn)	Div. 2. Small (Kapteyn).
Metallic flutings..	?	?
Carbon	?	?

We find that the gaseous stars are chiefly in the Milky Way and are far away from us; that the proto-metallic stars are not so confined to the Milky Way, and they are not so far away from us. But when we come to the metallic stars and the carbon stars they have not much obvious connection with the Milky Way, and they are close to us. Unfortunately, with regard to the metallic fluting stars the information is not so complete. Mr. McClean has dealt with a very small number, and he shows that they, like Dunér's stars, the carbon stars, have very little relation to the Milky Way. We thus obtain a tremendous separation between the hot stars with their great distance and the cooler stars with their smaller distance.

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* Kapteyn finds small proper motions for gaseous and proto-metallic stars, but does not separate them into two groups.

Although this discussion of the distribution of different types of stellar spectra indicates a collective tendency of some types, it proves at the same time that the chemical substances represented in such types are distinctly not limited to the regions in which they predominate. Thus we know of hydrogen in all stars except the carbon stars; although the stars showing strong indications of helium are most numerous in and about the Milky Way, stars of this kind do appear in other parts of space remote from the Milky Way, among them being the bright stars Spica and η Ursæ Majoris. Besides this direct evidence of the wide diffusion of helium there is the indirect evidence based upon the fact that helium is known to be present in the sun although it is not represented among the Fraunhofer lines. By analogy then we must allow that helium is also present in Arcturus and the thousands of other stars which have spectra like the sun which have no special connection with the Milky Way. Helium must, therefore, be practically like hydrogen, distributed in all directions as seen from the sun.

Another illustration of this general diffusion of a particular kind of matter is afforded by carbon. In the hottest stars, stars like the sun, and the coolest stars, we alike find indications of this substance, so that a localisation of any particular type of star does not imply the restriction of carbon to such localities. Again, if we take iron, we find its indications, either as iron or proto-iron, through a great variety of stellar types, while we may say that calcium and magnesium show direct evidence of their presence in almost every star.

Thus we are led to conclude that there is no localisation of the chemical elements so far as *direction* in space is concerned. While the discussion of proper motion indicates that particular types of stars tend to congregate at distances peculiar to themselves, the condensation is by no means absolute. Some stars of each type have proper motions widely different from the average. Hence at all *distances* from us we find similar chemical types of stars and therefore evidence of similar chemical substances.

We have already seen that the chemistry is the same in all *directions*, so that, finally, we must grant that the chemistry of all parts of space is the same. In other words the chemical parishes required by the view that the stellar types represent different chemical conditions as regards the presence or absence of certain substances do not exist. In no direction from our system, in no shell surrounding it, is any chemical element found which is not present in other directions and in other shells.

The major objection then against the stellar evidence in support of the dissociation hypothesis, upon inquiry, vanishes into thin air.

Our lengthened consideration of this question has really led us to a firm support not only of the dissociation hypothesis but of the meteoritic hypothesis as well.

As on the latter hypothesis the stars become hot in consequence of meteoritic collisions, we should expect to find nebulous conditions following suit; seeing that nebulæ are masses of meteorites, we should expect to find especially the gaseous nebulæ and results depending upon their presence in the region where the hottest stars exist in which dissociation has been studied.

The planetary nebulæ consist of streams of meteorites moving generally in spirals or in circular paths. There is no very great disturbance; we get a bright line spectrum from them, and we know they are practically limited to the Milky Way. We have found that the bright-line stars are limited to the Milky Way; they are simply stars involved in nebulæ. There again we get a connection between the Milky Way and nebulæ. The new stars are due to relatively fixed nebulæ driven into by moving nebulæ comet fashion, and they are also limited practically to the Milky Way; there again we have the nebulous touch. The nebulous regions, which Sir William Herschel was the first to chronicle, are more prevalent near the Milky Way than elsewhere.

It will be seen that we have a strict association of nebulæ, possible dissociation conditions, and the hottest stars in which that dissociation has been studied; and we are at length face to face with a simple explanation of the close contiguity of these apparently very diverse phenomena.

CHAP. XVIII.—REPLIES TO SPECIAL OBJECTIONS.

I NOW proceed to consider some less general objections. When I brought the question of dissociation before the Royal Society in 1897, in a discussion which I was requested to initiate, I pointed out that it had been proposed to explain the spectral differences between such stars as Bellatrix with its hydrogen and cleveite gases; Sirius with its tremendous development of hydrogen; and our own sun and stars like it with an atmosphere chiefly metallic; by supposing that "the hydrogen and cleveite gases may from some reason or other escape from among the metallic vapours and form an upper special atmosphere of their own, in which, in consequence of its greater chemical simplicity, the lines of these substances will become more important,"* and I added, "But this argument is not philosophical, because we have no right to assume such a change."†

This remark, referring to a very special point, was unfortunately misheard, and Dr. Schuster in the discussion stated:—

"Had Mr. Lockyer confined himself to bringing forward his hypothesis as one which is legitimate, consistent, and deserving of attention, many of us would I think have agreed that he had made out a good case. But he claims his theory as the only one which can explain the facts, and dismisses as unphilosophical the only alternative which he discusses."

In spite of this misapprehension, however, Dr. Schuster's criticisms are of great value, and I propose to consider them in this place and reply to them as best I can. I may add that he expresses his concurrence with my system of classification; and the necessity of a constant appeal to laboratory experiment is insisted upon; at the same time he acknowledges that the investigation of the enhanced lines is "a very material advance."

In my paper I pointed out, in relation to stellar atmospheres, that what we might expect to observe if we assumed the sun's temperature to be increased would be vastly different according as dissociation did or did not take place (see pp. 78-9). I said:—

"*The only change which we can imagine on the usual hypothesis, as resulting from the increase of temperature, is that with the increase in*

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* *Proc. Roy. Soc.*, vol. lxi, p. 202.

† *Loc. cit.*

volume there will be a reduction in density, and all the lines will be equally enfeebled. But this is exactly what does not happen."

With regard to this statement Dr. Schuster writes:—

"With this remark I cannot agree. The main fact to be explained is the gradual displacement of hydrogen, which is predominant in the hottest stars, by calcium, iron, and other metals. There are in my opinion several causes at work which might produce that effect. A glowing mass of gas may be either in thermal or in convective equilibrium, and the spectroscopic appearances in the two cases will be profoundly different. In reality an intermediate state probably is arrived at, but there is good evidence to show that the state of convective equilibrium is more nearly approached in our sun than in the hydrogen stars. We know as a fact that there are powerful convection currents near the sun's surface. There is, in consequence, an approach to a uniform distribution of matter and enormous differences of temperature in layers which are comparatively close together. Those who have not given much attention to this subject will hardly realise the differences of temperature brought about by convection currents. On the surface of the sun the temperature gradient produced by convection currents would be equal to 20,000° for each 100 kilometres difference in level, so that an angular distance of one second of arc would correspond to a difference of 100,000 . Radiation and condensation will diminish this gradient, but that it is very large is sufficiently proved by the spectroscopic evidence. Thus, according to the results of Messrs Jewell, Mohler, and Humphreys,* the pressure in the reversing layer for hot calcium giving the H and K lines is about six atmospheres, while that for the cooler calcium vapour is about three atmospheres. With a gravitational constant twenty-seven times as large as that of our earth, a difference of three atmospheres can only mean a comparatively small difference in level; while, then, in the sun we must admit a more or less effectual stirring up of the constituents together with an accompanying rapid temperature gradient, the evidence is just the other way in the case of stars like γ Lyræ. The spectrum of that star, according to Professor Lockyer, contains only the high temperature lines of iron. This means not only that the reversing layer is very hot, but also that there are no rapid changes of temperature at different levels. It is impossible to imagine this hot layer of gas ending abruptly; it must be surrounded by cooler matter, which cannot be iron, as the low temperature lines of iron do not appear. In such a star there cannot be an effectual mixing up of the constituents, and hence the layers of gas will arrange themselves according to the laws of diffusion. It would follow that hydrogen, being a lighter gas than iron, will be chiefly represented in the cooler and outer layers, while iron will be found more particularly in the inner and hotter parts. The relative proportion of different elements in different layers will be regulated partly by their density, but to a great extent also by the total quantities present in the star; for the different gases will not float on each other as liquids might, but the density of each gas will increase steadily from the surface to the centre. The chief difference, according to this view, between a hydrogen and a solar star lies in the more or less effectual mixing up of the constituents. If we could introduce a stirrer into γ Lyræ there can be no doubt whatever that the low temperature lines of iron would make their appearance, while, on the other hand, if we could stop all convection currents on the surface of the sun the hydrogen which now lies under the photosphere would gradually diffuse out and give greater prominence to its characteristic absorption lines."

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* *Astrophysical Journal*, vol. iii, p. 138.

"In the face of the direct evidence of the absence of convection currents in the hotter stars, it is not necessary for the purpose of my argument to discuss why this is the case, but it can be seen that diminished gravity, diminished density, and consequently increased viscosity, will contribute to the effect, while effectual radiation will, owing to the smaller density, take place more evenly through a thicker layer of the envelope, so that the principal cause of convection currents will also be much diminished."

In replying to this objection of Dr. Schuster's I will first deal with the convection currents and the tremendous temperature gradient which Dr. Schuster postulates. In the sun, the seat of such convection currents, according to him, while they are absent from γ Lyræ they are sufficiently powerful to cause a difference of 20,000° C. for each 100 kilom. in difference of level, or, as he otherwise puts it, a difference of 100,000° for one second of arc.

The eclipse photographs give no evidence of the rapid temperature gradient in the sun supposed by Dr. Schuster. In the Indian series, two successive photographs taken at intervals of about one second near the beginning of totality differ inasmuch as the first includes a stratum about 150 miles above the photosphere, which would be covered by the moon when the second was taken (except for the effect produced by irregularities in the moon's limb). Yet there is no great difference in the spectra; both contain arc and enhanced lines about equally, and therefore indicate that the temperature changes can only be small in a depth of 150 miles. In fact throughout a distance of 500 miles above the photosphere the spectrum indicates no change of temperature of importance.

We have got the facts then in the eclipse photographs, and find no large spectral changes in a region where Dr. Schuster postulates a difference of 100,000° C. Are we to take this value as the temperature of the sun's photospheric level? If so, how does Dr. Schuster reconcile it with the values obtained by all the recent workers who make it less than 10,000° C.? and even with Homer Lane's 28,000°?

Surely the facts show that there are not, in the sun, such tremendous convection currents as are demanded on Dr. Schuster's view.

Professor Schuster refers to the conclusion drawn by Messrs. Jewell and others as to the pressure of hot and cold calcium in the reversing layer. His reference shows that he agrees with my view that we are dealing with different molecules, but I wish to remark that I think we must not be too hasty in accepting the conclusions to which he refers, for the reason that the eclipse photographs do not tally with them at first sight. In these photographs (1898) the K layer reached a height of 6,000 miles; the λ 4226·9*b* layer only 2,000 miles. This suggests that cool calcium falls and is dissociated at the bottom. It certainly does not mean that there is a layer of cooler calcium at a higher elevation and at less pressure surrounding a hotter one at a lower elevation and higher pressure.

The evidence on which it is assumed that convection currents are absent from the hotter stars like γ Lyræ of decreasing temperatures does not appear to be conclusive. But let us assume it.

The absence of cool iron lines only shows that we are in a region of higher temperature than in the sun. May there not still be a rapid temperature gradient, from "high" to "very high" temperature instead of from "low" to "high" as in the sun? But in any case, a mere stirring up of γ Lyræ would not make its spectrum like that of the sun. Such stirring up could only introduce the cooler lines of iron if the proto-iron were by that process driven out into the cooler regions, where it might become iron and so produce cool iron absorption lines in the spectrum of the star. But it by no means follows that these cool iron lines would be as strong as in the solar spectrum, for we know that the amount of absorbing proto-iron is only small. Moreover, this process of stirring would hardly reduce the intensity of the hydrogen lines.

A reduction of temperature, however, furnishes us with a sufficient explanation of the changes observed in passing from such a star as γ Lyræ to one like the sun; the cool lines of iron would appear as a matter of course, and such lines would become stronger if iron can be formed at the expense of the hydrogen.

If we take the converse view, and suppose the postulated convection currents in the sun to be stopped, I do not see how such a condition of things would result in changing the present spectrum of the sun into a spectrum like that of γ Lyræ. We have not only to explain the increased intensity of the lines of hydrogen, but the appearance of the enhanced lines of iron as absorption lines. Now these enhanced lines are already in the sun's chromosphere, and are presumably absent from the Fraunhofer spectrum, because the vapour producing them approaches the temperature of the photosphere. Is it possible that a state of quiescence in the sun would so increase the temperature of the photosphere as to make visible the absorption of these high temperature vapours? And, if this be possible, there would still be no apparent reason for the disappearance of the cool lines of iron. The change, however, from γ Lyræ is readily explained if we grant that there is an increase of temperature, producing proto-iron from the previously cool iron vapour, and a dissociation capable of producing the observed increase of hydrogen absorption at the expense of proto-iron.

How the increased absorption of hydrogen can be accounted for otherwise is not clear. The idea of hydrogen being set free for this purpose from beneath the photosphere does not seem to me probable.

The final discussion of such subjects as these is very difficult, because we learn from the sun that the absorption recorded is only that of a middle region. Neither helium nor coronium writes its record among the Fraunhofer lines. Surely everybody will agree that there are hundreds of substances in the higher cooler reaches of the solar atmosphere which write no record. How then can we say that under the conditions assumed by Dr. Schuster "there can be *no doubt** whatever that the low temperature lines of iron would make their appearance."

Dr. Schuster also refers to hydrogen "imprisoned beneath the photosphere"; is there any justification for this view? The complete history of hydrogen, including proto-hydrogen in stellar atmospheres, is simply and sufficiently explained on the dissociation hypothesis. I question whether an explanation which requires such an imprisonment of hydrogen is more satisfactory.

I now proceed to give another quotation from Dr. Schuster:—

"There is especially one question which Professor Lockyer must be prepared to answer. Amongst the heavier metals, tellurium, antimony, mercury, are not represented in the sun, but they are found in Aldebaran. To be consistent, we must, if we adopt the theory of dissociation, assert that these metals are decomposed in the sun. But, if I understand Professor Lockyer right, he believes that with our strongest sparks we can exceed the state of dissociation which exists in the reversing layer of the sun. Take such a strong spark, then, from a pole of mercury, do you get lines of helium, or of calcium, or of hydrogen? This seems to me to be almost a crucial experiment. Possibly, of course, we should get high temperature lines not hitherto looked for, but present in the sun. If so, the objection would fall to the ground, but if this is not the case, and if mercury at a high temperature refuses to be dissociated into simpler elements, a most serious objection to the theory would have to be answered."

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* The italics are mine.—N. L.

In reply to this I may state that in recent large dispersion photographs the differences pointed out by Dr. Schuster between the spectra of the sun and Aldebaran do not exist. I quite agree that such experiments as he describes should be made, and I have made many, but the work which is necessary has been interrupted, since I have no longer at my disposal the Spottiswoode coil, the superiority of which, over all others, for such a general inquiry as this I have amply demonstrated. I may say here, however, that so far as the observations have gone there is apparently an agreement between the laboratory and stellar results, but there are possible sources of error which require to be studied, and also in a matter of such high importance the experiments must be repeated many times before a final statement is made.

Dr. Schuster next states:—

"While I think that we shall all admit that different stars are in different stages of development, and that hydrogen stars will ultimately approach more nearly to the state of our sun, it would be unwise to push the argument of uniformity too far, and to say that every star will pass exactly through the same stages. Ritter, who is favourably inclined to the dissociation hypothesis,* gives good reason to believe that the sun's surface was never much hotter than it is now, and that the higher temperature of hydrogen stars is connected with their greater masses. It is, in fact, impossible to admit that the process of development should be quite independent of the total mass of the star. It may be urged that Arcturus must have a mass much larger than that of our sun, and its spectrum, according to Professor Lockyer, is identical with that of the sun. But I suppose that that statement only refers to the blue and violet region, for, according to Dr. Huggins, to whose early stellar photographs we owe so much, the spectrum of Arcturus in the ultra-violet approaches that of Sirius."

Although the masses of very few white stars have been determined with trustworthy results, one case in which a white star can be shown to have a smaller mass than the sun will be sufficient to show a weakness in Ritter's conclusions. For β Persei (Algol) Vogel states the mass as four-ninths that of the sun; so that the sun, on Ritter's theories, may be supposed to be of sufficient mass to reach a temperature as high as that of β Persei—a result which does not accord with his statement that the sun has probably never been, and never will be, much hotter than at present.

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* *Wied. Annalen*, vol. xx, p. 152.

Sir William Huggins's statement as to the ultra-violet spectrum of Arcturus is most interesting, if confirmed. The Kensington series of large dispersion photographs show an almost perfect similarity of spectrum with that of the sun, extending to λ 3880.

It is difficult to see any objection, on the ground of unequal masses, even if we grant the similarity of the two spectra. It is only necessary to suppose that Arcturus, like the sun and other solar stars, has passed its hotter stages, and that it may have commenced its condensation before the sun.

To take another case, ζ Ursæ Majoris and β Aurigæ have spectra which are almost identical, although the masses of the two systems, according to Pickering, are respectively 40 times and 4·6 times that of the sun. Another very hot star, Spica, has a mass only 2·6 times that of the sun.

Dr. Schuster further suggests that it is not known to me that Ritter has long studied the question of gaseous masses contracting under their own gravitation. In my work which has consisted in the discussion of spectroscopic observations, I was at the outset led to the view that it was not a question of gaseous masses at all, originally, and therefore I did not refer to Ritter's conclusions on this point. Again, I had to face the spectroscopic evidence of a chain of obviously cooling bodies, and it was a detail to consider the fact that "a radiating and contracting mass is not necessarily a cooling mass," because in spite of this truism a time must certainly come when all bodies will find their temperature reduced. I am aware that Ritter's conclusions regarding the first rise and subsequent fall of temperature of gaseous bodies, are similar to those supported by the spectroscopic evidence of what I have considered to be condensing swarms of meteorites, but it would not have been fair to claim Ritter's conclusions as supporting my own, because the bases of the phenomena considered by us were so different.

I, perhaps, may be allowed to point out that where Ritter's conclusions do not seem to harmonise with the spectroscopic facts, it may be that, as Professor Perry has pointed out,* a stellar atmosphere is a more complicated thing than the theory of a gaseous mass implies. Even the spectroscope deals generally only with the reversing layer.

Professor Perry writes:—

"He (Ritter) assumes that the radiating layer on the outside of a star is of constant mass. He also assumes that the rate of radiation is proportional to the fourth power of the average temperature of this layer. He is dealing with temperatures which are so much greater than the temperatures with which we work in the laboratory, that such assumptions must be regarded as quite arbitrary.

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* *Nature*, vol. lx, p. 247, 1899.

"Mr. Homer Lane, in his classical paper on the theoretical temperature of the sun,* makes the assumption that Dulong and Petit's law of radiation is true for solar radiation, and he uses it to calculate the temperature of the radiating layer, which he finds to be 28,000° F. That is, he uses an empirical law, obeyed possibly at laboratory temperatures in radiation from hot solids, to express the radiation at enormous temperatures from a hot layer of gas which has layers of gas of all sorts of temperatures above and below it.

"It seems to me that we know too little about the phenomenon of radiation from layers of gas with denser and hotter layers below and rarer and colder layers above to allow of any weight being placed upon these assumptions of Ritter or Homer Lane. In a star we have layers of fluid at all sorts of temperature and density. We have no laboratory knowledge of radiation that is applicable. We know very little about any star except our own sun. * * * Assumptions like those of Homer Lane and Ritter may lead to results which are altogether wrong."

Finally, I may refer to two more objections from another quarter. the first relates to the connection which I have insisted upon between the length of the continuous spectrum and the temperature of the light source, and I have stated that this is based upon Kirchhoff's law. To this it is objected that rays far up in the ultra-violet can be emitted from bodies not at a high temperature. The inference is that the stars with the longest spectra may be cold. But they are connected with the sun by an unbroken chain of sequences in the phenomena. Then is the sun also cold?

Again, it is urged that the phenomena of the gaseous stars instead of being due to high temperature, are caused by phosphorescence. Where then are Crookes's phosphorescent spectra? If this objection implies that hydrogen can be made to phosphoresce so as to give us Pickering's spectrum, the objector should have made the experiment before he committed himself to such an objection.

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* *American Journal of Science and Arts*, 2nd series, vol. i, p. 57, 1870.

BOOK V.—INORGANIC EVOLUTION.

CHAP. XIX.—WHAT EVOLUTION MEANS: ORGANIC EVOLUTION.

IN the previous chapters I have endeavoured to correlate all the facts which have been obtained during the last, let us say, thirty years, in relation to the sun, with more recent facts that have been gathered with regard to the stars. In this we were, by hypothesis, watching the effects of dissociation as the temperature rose higher and higher; we have found that the dissociation hypothesis, the view, namely, that at high temperatures the chemical units with which we work at low temperatures are broken up into smaller masses, explains the spectral phenomena observed not only in our laboratories but in the sun and stars.

I have also shown that in the opinion of many investigators such a dissociation is necessary to explain the phenomena observed in physical inquiries other than those which directly concern us here.

In these concluding chapters I propose to change the point of view, to consider the phenomena no longer from the point of view of dissociation but from that of *evolution*.

What is evolution? To answer this question I can refer to another line of work in which the word is frequently used and thoroughly understood. It is important that I should do this for another reason, which will be gathered later. That line of work has to do, not with inanimate forms, like the chemical elements and the stars, but with living things, with so-called organisms. Most of my readers know that what we now recognise as one of the greatest triumphs of the century just ending was the determination of the truth of a so-called "organic evolution" in which we have, I suppose, the most profound revolution in modern thought which the world has seen.

That evolution tells us that each kind of plant and animal was not specially created, but that successive changes of form were brought about by natural causes, and that the march of these forms was from the more simple to the more complex. Organic evolution, in fact, may be defined as the production of new organic forms from others more or less unlike themselves; so that all the present plants and animals are the descendants, through a long series of modifications or transformations, or both, of a limited number of an ancient simpler type. We must not suppose that this change has gone on as if things were simply mounting a ladder; the truth seems to be that we have to deal with a sort of tree with a common root and two main trunks representing animal and vegetable life; each of these is divided into a few main branches, these into a multitude of branchlets, and these into smaller groups of twigs.

This new view represents to us the evolution of the sum of living beings; shows that all kinds of animals and plants have come into existence by the growth and modification of primordial germs. Now I want just to say that this is no new idea, it is the demonstration which is new to us in our present century and generation; we have really to go back to the seventeenth century, if indeed we must not go as far back as Aristotle, for the first germs of it; but with regard to the history, however, I have no time to deal with it. There are two or three points, however, to be considered in regard to this evolution. The individual organic forms need not continuously advance, all that is required is that there shall be a general advance—an advance like that of our modern civilisation—while some individual tribes or nations, as we know stand still, or become even degenerate. With this reservation, the first forms were the simplest. It may be that as yet we know really very little of the dawn of geological history; that the fossiliferous rocks are nowhere near the real base. This conclusion has been derived by Professor Poulton* from the complexity of the forms met with in them; still we find that we have not to deal with such a vast promiscuous association of plants and animals of lowest and highest organisation as we know to-day; we deal relatively only with the simplest. The story both with regard to plants and animals is alike in this respect.

Let me deal with the plants first. The first were aquatic—that is to say, they lived in and on the waters. So far as we know, the first plant life was akin to that of the algæ, which include our modern seaweed, moss-like plants followed them, and then ferns, and it is only very much later that the forms we know as seed plants with gaily coloured flowers living on the land made their appearance. The general trend of change amongst the plants has been in the direction of a land vegetation as opposed to one merely in or on the surface of the waters, and some present seaweeds exhibit the initial simplicity of plant-structure which characterised the beginning of vegetable life, while the seed plants I have mentioned are of comparatively late development; but we still have our seaweed; so that with all the change in some directions, some forms like the earlier survive.

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* Presidential Address, Section D, British Association Meeting at Liverpool, 1896.

After this explanation, relating to work in an apparently different direction, there should be no difficulty in understanding the meaning I attach to the word "evolution" so far as the history of plant change is concerned, in relation to the chemical elements; but we are not limited to plant life. The same conceptions apply to animal life, and it is important for my subject that I should refer to that also. What do we find there? We are brought face to face with the same progression from simple to complex forms. This is best studied by a reference to the geological record.

Stratigraphical geology is neither more nor less than the anatomy of the earth,* and the history of the succession of the formations is the history of a succession of such anatomies; or corresponds with development as distinct from generation. In stratigraphical geology, as can be gathered from any book on the subject, we find the names of certain beds which contain certain different forms of animal and vegetable life. We begin with the Laurentian and Algonkian and then pass to the Cambrian, then to the Ordovician, the Silurian and Devonian, and so on through a long list of beds and geological strata until we come eventually to the Recent, that is to say, the condition of things which is going on nowadays on the surface of the earth. And if we prefer to map those many different beds into more generic groupings, we begin with the Primary or Palæozoic, we pass on to the Secondary or Mesozoic, and then we finally reach the Tertiary or Cainozoic. The deposition of these beds and of the animal life which has been going on continuously on the surface while those beds have been deposited, gives us the various changes and developments which have taken place with regard to animal forms.

It is worth while to go a little more into details and to indicate the changes in these forms which have taken place, in the most general way. Beginning with the Lower Cambrian, we find that the animal forms were represented by Invertebrata such as Sponges, Corals, Echinoderms, Brachiopods, Mollusca, Crustacea with many early Trilobites; not to mention true Fucoids and other lowly plant-remains. When we come to the Silurian, we find a large accession of the above forms, especially of Corals, Crinoids, and Giant Crustaceans (such as *Pterygotus*) and armoured animals (Ostracodermi) without a lower jaw, or paired fins; the beginnings of Vertebrate life, not yet fully evolved, and one lowly organised group of armoured fishes named *Cyathaspis* (without bone-cells in their shelly-shield). Here, too, we meet with the first air-breathers; the wing of a Cockroach, and several entire and undoubted Scorpions! Thus in addition we get vertebrates as opposed to invertebrates, and the first traces of the fishes. In the advance to the Devonian the fishes (associated with giant Crustacea) predominate; it has been called the age of fishes. In the next series, the Carboniferous, we find the first certain traces of amphibians, of which the early existence is like that of a fish: a state of things illustrated by the frog, which the majority of us in our early days have, I am sure, studied as a tadpole in its early stages; and some of these amphibians still retain fish-like characters. It is not until we arrive at the Permian that the true reptiles are met with, but in the next great series, the Triassic, we meet with a remarkable evolutionary group of Reptiles, the Theriodontia, or beast-toothed animals, because (unique among reptiles) they possess a dentition like a dog or a lion, with incisors, canines and cheek teeth; the precursors, doubtless, of the succeeding mammalian type. We pass easily thus from the reptiles to mammals which are related to them; for instance, the ornithorhynchus and the echidna are both Australian mammals which bring forth their young within the egg as do the reptiles. After that we begin to deal with birds. The early birds were strikingly reptilian in some of their characters; and the pterodactyle, remains of which exist in many museums, was really a winged reptile and not a bird. From that we gather that mammals and birds are variants of reptiles. When we progress from the Jurassic to the Recent, we find man making his appearance as a direct descendant of all those early forms.

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* Huxley, *Q.J.G.S.*, vol. xxv, p. 43.

When we come to study the life-history of the various forms brought before us by the geological beds, we find it to vary considerably, a fact indicated by the presence or absence of the different genera in the various strata. We find that the trilobites, for instance, only appear in the very early geological formations; there is no trace of them in the recent, but of the annelids and Brachiopods we note that they are continuous from the earliest to the latest formations; we still have our worms. Again we learn that certain other organic forms made their appearance very low down in the time scale, forms which were not represented at all in the earlier Cambrian and Silurian, and that some of these are continuous to the present day.

Let us take the story of the fishes. A great many fishes made their appearance at the Devonian stage, there were few in the Silurian; some of these stopped there, whereas others have been continued from the Devonian times to our own. Take, for instance, the Australian mudfish *Ceratodus;* to judge from the teeth this fish might well have lived on unchanged from late Palæozoic times until the present day! We see there is a tremendous variation of possible life-range, so to speak, with regard to these different forms. In that way, then, the geologist has been able to bring before us the continuity of life in various forms, from the most ancient geological strata to the most recent. The record may be incomplete, but is complete enough for my purpose.

But that is not the only evidence of evolution to which I can refer. The teachings of embryology confirm the argument based upon the study of geology, and suggest that the life-history of the earth is reproduced in the life-history of individuals. The processes of organic growth or embryonic development present a remarkable uniformity throughout the whole of the zoological series; and although knowledge is still limited, some authorities hold that there is the closest possible connection between the development of the individual and the development of the whole series of animal life. There are others, however, who do not regard the argument derived from embryology as a very convincing one. However this may be, if we study the embryos of the tortoise, fowl, dog, and man, we find that there is a wonderful similarity between them at a certain stage. At a further stage of development the similarity is still borne out. This does not mean that a vertebrate animal during its development first of all becomes a tortoise, and then the various animals which are represented by these embryos; it simply means that they are all related, inasmuch as there is continuity.

After these references to plants and animals it should be clear what organic evolution really is, and therefore what evolution is generally.

CHAP. XX.—THE STELLAR EVIDENCE REGARDING INORGANIC EVOLUTION.

JUST as plants and animals compose the organic or living world, so do the so-called chemical elements (either single or combined), compose the inorganic or non-living world.

Formerly plants and animals and the chemical elements were all considered to represent special creations—"manufactured articles"; we now know that plants and animals do not; that they have been continuously evolved from simpler forms.

What we have now to consider is whether the facts set out in the preceding chapters do or do not indicate that we must accept the chemical elements, like plants and animals, as products of evolution.

Taking plants and animals as we know them, the more we dive into past times the more differences in form are noted, though the temperature at which the vital processes were and are carried on have certainly not been widely different.

Taking the chemical elements as we know them here, we find differences in composition continuously indicated as stars of successively higher temperature are studied. It is obvious that this is a very important point. In inorganic evolution we are dealing with a great running down of temperature; how tremendous no man can say. We know the temperature of our earth, but we do not know, and we cannot define, the temperatures of the hottest stars. So that how great the temperature of the earth may once have been, supposing it to be represented by the present temperature of the hottest star, no man knows with certainty.

With regard to organic evolution, however, which has to do with the plant world and the animal world, there can have been no such running down of temperature at all. The temperature must have been practically constant within a very few degrees.

The differences then depend upon time in organic, and upon temperature in inorganic, nature.

It is for this reason that in the inorganic evolution which now concerns us the chemical changes brought about by changes of temperature must be our chief guide, and the earliest and simplest forms must be sought in regions where the highest temperature is present.

The effect of high temperature in producing simplifications is known to everybody. If we deal, for instance, with well known chemical compounds, say chloride of sodium, that is common salt, and oxide of iron, that is iron-rust, we produce the simpler substances of which they are composed by heat, and we further have no difficulty in recognising the fact that chlorine and sodium in one case, and oxygen and iron in the other, must have existed before their compounds, common salt and iron-rust, could be formed or associated. Water is split into hydrogen and oxygen at a high temperature, so that there is a temperature above which the two gases would remain in contact but uncombined; when the temperature falls water is produced. Dissociation, therefore, in all its stages must reveal to us the forms the coming together of which has produced the thing dissociated or broken up by heat. *If this be so, the final products of dissociation or breaking up by heat must be the earliest chemical forms.* Hence if the various stars behave like the various geological strata in bringing before us a progression of new forms in an organised sequence, we must regard the chemical substances which visibly exist in the hottest stars which, so far as we know, bring us in presence of temperatures higher than any we can command in our laboratories, as representing the earliest evolutionary forms.

I have said *if*. Now do the stars from the hottest to the coldest present us with a progression of new forms as the geological strata do from the oldest to the newest?

The preceding pages enable us to answer this question fully. On p. 47 I indicated how, in cosmical evolution, we deal with a continuity of effects accompanied by considerable changes of temperature; from the gradual coming together of meteoritic swarms until eventually we have a mass of matter cold and dark in space. The various stars which represent the different changes have been got out and have, in fact, been arranged along a so-called temperature curve. As we ascend one branch of this curve the stars get gradually hotter and hotter till ultimately at the top we find the hottest stars that we know of. Then on the descending branch are represented the cooling bodies, and finally they come down in temperature until we reach that of a dark world like the companion of Sirius, of our own moon, and the planet in which we dwell.

Thanks to the recent work, we can now deal with all these bodies in special relation to their chemistry. No doubt the record will be made more complete as time goes on and other workers come into the field; but it is already complete enough for my present purpose, for the story is one of changes of chemical forms from one end to the other.

When the photography of stellar spectra work was begun our knowledge was so incomplete that a continuous chain of chemical facts was out of the question; but, thanks to the recent advances, we can deal with this inorganic evolution from a chemical stand-point, and what we have now to do is to consider the result of this inquiry.

Chapters VI and VII give the evidence on which the statement can now be firmly made, that in the hottest stars we are brought in presence of a very small number of chemical elements. As we come down from the hottest stars to the cooler ones the number of spectral lines increases, and with the number of lines the number of chemical elements. I will only refer to the known substances—it looks as if at present we have still many unknowns to battle with. In the hottest stars of all, we deal with a form of hydrogen which we do not know anything about here (but which we suppose to be due to the presence of a very high temperature), hydrogen as we know it, the cleveite gases, and magnesium and calcium in forms which are difficult to get here; we think we get them by using the highest temperatures available in our laboratories. In the stars of the next lower temperature we find the existence of these substances continued in addition to the introduction of oxygen, nitrogen, and carbon. In the next cooler stars we find silicium added; in the next we note the forms of iron, titanium, copper, and manganese, which we can produce at the very highest temperatures available in our laboratories; and it is only when we come to stars much cooler that we find the ordinary indications of iron, calcium, and manganese and other metals. All these, therefore, seem to be forms produced by the running down of temperature. As certain new forms are introduced at each stage, so certain old forms disappear.

The salient features of the organic record are thus exactly reproduced, to such an extent indeed that the most convenient way to present the results was to define the various star-stages by means of the chemical forms which they reveal to us in exactly the same way as the geologists have done in regard to organic forms; so that we may treat these stellar strata, so to speak, as the equivalent of the geological strata.

From the hottest to the coldest stars I have found ten groups so distinct from each other chemically that they require to be dealt with separately as completely as do the Cambrian and the Silurian formations. Imitating the geologist still further, I have given names ending in *ian* to these groups or genera beginning with the hottest, that is the oldest dealing with the running down of temperature:—These are Argonian, Alnitamian, Achernian, Algolian, Markabian [a "break in strata"], Sirian, Procyonian, Arcturian (solar), Piscian.

I have also defined the chemical nature of these stellar strata as the geologist defines the nature of any of his various beds; we can say, for instance, that the Achernian stars contain chiefly hydrogen, nitrogen, oxygen, and carbon, and to a certain less extent they contain proto-magnesium, proto-calcium, silicium, and sodium,* and possibly chlorine and lithium; so that at last, by means of this recent development of spectrum analysis, we have been able really to do for the various stars what the biologist, a good many years ago, did for the geological strata.

It will be seen, then, that the answer to the question: "Do the stars show a progression of chemical forms as the geological beds show a progression of organic forms?" is clear and precise. There is a progression.

We are justified, therefore, in considering the matter further from the evolution point of view. There are several points which merit detailed consideration.

Obviously we cannot expect to get much help by thinking along several obvious lines, for the reason that in the stars we are dealing with transcendental temperatures. For instance, we must not make too much of the difference between gases and solids, because at high temperatures all the chemical elements known to us as solids are just as gaseous as the gases themselves; that is to say, they exist as gases; at a high temperature, everything, of course, will put on the nature of gas. Those substances with the lowest melting points, such as lithium and sodium, will, of course, under our present conditions put on the gaseous condition very much more readily than other substances like iron and platinum, but those are considerations which need not be taken into account in relation to very high stellar temperatures; of course, there would be no solids at a temperature of 10,000° C., and there will be no gases in space away from the stars if the temperature of space be taken at absolute zero.

Then with regard to metals and non-metals. Here again we really are not greatly helped by this distinction. The general conception of a metal is that it is a solid, and that, therefore, a thing that is not a solid is not a metal: but the chemical evidence for the metallic nature of hydrogen has been enlarged upon by several very distinguished chemists, and mercury is generally known as a liquid. With regard to non-metals, there are certainly very many. Carbon is supposed to be a non-metal, and it is remarkable that, so far as the stellar evidence has gone as yet carbon seems to be the only certain representative of that group.

I must point out specially that the table of the chemical definitions of the various stellar genera (given on pp. 70 and 71), which contains nothing but hard facts, is perhaps, like the geological record, more important on account of what it indicates as to the presence of the chemical elements in the stars than it is for what it omits.

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* Campbell, *Astronomy and Astro-physics*, 1894, vol. xiii, p. 395.

There are a great many reasons why some of the substances which may exist in these stars should not make their appearance. I wish to enlarge upon the fact that, seeing the very small range of our photographs of stellar spectra, seeing also that it does not at all follow that the crucial lines of the various chemical substances will reveal themselves in that particular part of the spectrum which we can photograph, the negative evidence is of very much less importance than the positive evidence. I think it is possible, for instance, that we must add lithium to the substances which we find in the table on pages 70 and 71, we must certainly include sodium and also aluminium, and chlorine possibly, but about sulphur at present I have no certain knowledge. At all events, we can with the greatest confidence point out the remarkable absence of substances of high atomic weight, and the extraordinary thing that the metals magnesium, calcium, sodium and silicium undoubtedly began their existence in the hottest stars long before, apparently, there is any obvious trace of many of the other metals which a chemist would certainly have been looking out for.

Chap. XXI.—The Simplest Elements appear First.

With regard to the substances which appear in the hottest stars, the all important, the first point to make, is that the chemical forms we see are amongst the simplest.

How can this be determined? In two ways. The chemist will acknowledge that an element of low atomic weight is simpler, that is, has less mass than an element of high atomic weight. If we rely upon spectrum analysis we can say, when dealing with the question of "series," that the elements which most readily give complete series are in all probability simpler than those which give none, and this is still truer when we find that all the lines in the spectrum of a substance can be included in those rhythmical series, as happens in the case of hydrogen and the cleveite gases. Judged then by these standards it is certain that the first stage of inorganic evolution, if there has been such an evolution, is certainly a stage of simplest forms as in organic evolution, whatever view we take of the nature of the "atom."

It is worth while to compare in detail the results obtained by this newest form of spectrum analysis relating to "series," with the earliest stellar forms, because it is evident that we are here in presence of the beginning of a new method of study of the nature of the so-called chemical elements.

We found that the hottest stars contained hydrogen, helium and asterium. We have also found (Chap. X) that those substances have the simplest series; that is to say, one set of three. It is more than probable, although it is not absolutely established, that the lithium group of metals is also represented in stars of very high temperature. There, again, we have the simple series of one set of three. About sulphur we do not yet know positively, but it is probable, I think, that sulphur may exist in the hot stars. There, again, we get another simple set of three; so that for three perfectly certain constituents of the hottest stars, together with one present in all probability and one doubtful, we are dealing with the simplest series.

But now comes the remarkable fact that side by side with these simple substances we get in the hottest stars magnesium, calcium and silicium. Of the "series" conditions of the last we know nothing. Of magnesium and calcium only subordinate series have been determined.

We cannot suppose that the absence of the principal series means a greater simplicity, because I have shown that only about half the lines in the spectrum of each of these substances has yet been picked up in the series, and if the series represent the vibrations of a single particle, of course the lines which are not represented in the series, by theory must represent the vibrations of some other particles. So that there we are face to face with the possibility of a greater complexity of the particles which produce the series than of those which in the stars give us the lines not in the series. These then are other simple forms.

Coming further down in stellar temperatures we find oxygen; here we deal with six series instead of three, or two, as in the case of magnesium and calcium; and even then, as I have pointed out, we do not deal with above half the lines of the gas as we can see them at a higher temperature. This then, seems to suggest that in the hottest stars there are very various stabilities of very various forms: in short, there seems to be there as here distinctly the survival of the fittest; otherwise how can we account for the fact that certainly in the hottest stars we get three metals, magnesium, calcium and silicium, before we have indication of any other, and that where we have those metals and bring our series touch-stone to them, we find that instead of being very simple they are really very complex as they exist here. However this may be, we are now assured that there is a much greater quantity of some apparently more complex forms in the hotter stars than of the more simple ones; and that is a matter which the chemists, when they come to inquire into these questions which we are now considering, will certainly have to face. This suggests, too, another very interesting question. A great many simple organic forms appear in the stratigraphic series at a late period; some of the simplest forms died out, others remained. Now, it may be that some of the more simple forms in inorganic evolution, as in organic evolution, really represent later introductions; but, however this may be, it is perfectly certain that we have not an absolute parallel between the results of the spectroscopic observations of series and the spectroscopic observations of stars.

In all these changes we seem to be brought into presence of successive complications due to reduction of temperature, but there is a longer series of complications in some substances than in others. Of the origin of proto-magnesium and proto-calcium the stars as yet tell us nothing; but it is difficult to believe that the earliest forms of the other metals are not built up of some of the constituents of the heat ranges represented by those between γ Argus and α Crucis, and that their other complications began later.

The next point is that the astronomical record, studied from the evolution point of view, is in other ways on all-fours with the geological record in relation to increasing complexity. We note the same changes of forms, sudden breaks in forms, disappearances of old, accompanied by appearances of new, forms; and with these we have to associate, whether we consider the atomic weight point of view or the series point of view, a growth of complexity.

Although in this chapter I have chiefly referred to the stellar evidence, I must not neglect to point out that over a restricted range of temperature solar evidence can be utilized as well. We have brought the sun and the stars together into line in all matters relating to the discussion of the effects of higher temperatures. The photographs taken during the recent solar eclipses show that when we deal with the hottest part of the sun that we can get at, which is hotter than that part of the sun which produces the well-known absorption spectrum marked by the so-called Fraunhofer lines, we are not in an unknown territory at all, but are brought face to face with similar phenomena to those in the atmospheres of stars which are hotter than our sun. The bright-line spectrum of the sun's chromosphere seen during an eclipse shows us the effects produced by heat in the hottest part of the sun that we can reach; these we can compare with the dark lines of a star which contains absorption lines very different from those represented by the Fraunhofer lines, and we find that they correspond almost line for line.

Such an inorganic evolution was suggested by me many years ago now, to explain the few stellar facts with which we were then familiar. I must point out, however, that we are now in a very much better condition to consider this problem than we have ever been before, because at the present moment we have tens of thousands, I might almost say hundreds of thousands, of co-ordinated facts to go upon, and it is not a little remarkable that now the gaps in our knowledge have been filled up, we find ourselves in the presence of evidences of an evolution which is really majestic in its simplicity.

It is proper that I should say that just as the work of Darwin in the nineteenth century was foreshadowed by seventeenth century suggestions, so the stellar demonstration with which we are dealing has been preceded by hypotheses distinctly in the same direction. The first stage of chemistry was alchemy; alchemy concerned itself with transmutations, but it was found very early that the real function of the later science of chemistry was to study *simplifications*, and, of course, to do this to the utmost we want precisely those enormous differences in temperature which it appears the stars alone place at our disposal.

With regard to the general question of inorganic evolution, the first idea was thrown out in the year 1815 by Prout, who, in consequence of the low atomic weight of hydrogen, suggested that that substance was really the primary element, and that all the others, defined by their different atomic weights, were aggregations of hydrogen, the complexity of the aggregation being determined by the atomic weight; that is to say, the element with an atomic weight of 20 contained 20 hydrogen units; with an atomic weight of 40 it contained 40, and so on. The reply to that was that very minute work showed that the chemical elements, when they were properly purified and examined with the greatest care, did not give exactly whole numbers representing their atomic weights. They were so and so plus a decimal, which might be very near the zero point, or half-way between, and that was supposed to be a crushing answer to Prout's view. The next view, which included the same idea-- that is to say, a physical connection between these different things as opposed to the view that they were manufactured articles, special creations, each without any relation whatever to the other, was suggested by Döbereiner in 1817, and the idea was expanded by Pettenkofer in 1850. Both pointed out that there were groups of three elements, such as lithium, sodium, and potassium, numerically connected; that is, their atomic weights being 7, 23, and 39, the central atomic weight was exactly the mean of the other two, 7 + 39 = 46, divided by 2, we get 23. Another way, however, of showing that is that 7 + 16 = 23, and 23 + 16 = 39; the latter method suggests a possible addition of something with an atomic weight of 16.

In 1862 de Chancourtois came to the conclusion that the relations between the properties of the various chemical elements were really simple geometrical relations. It is not till 1864 that we come to the so-called "periodic law," which was first suggested by Newlands, and elaborated by Mendeléef in 1869. According to this law, the chemical and physical properties of the elements are periodic functions of their atomic weights. Lothar Meyer afterwards went into this matter, and obtained some very interesting results from the point of view of atomic volumes. He showed that if we plot the atomic volumes of the different elements, arranged according to their atomic weights from left to right, there is a certain periodicity in the apices of the curve indicating the highest atomic volumes.

So far there was no reference to the action of temperature in relation to this, but in 1873 I suggested that we must have a fall of temperature in stars, and that the greater complexity in the spectra of certain stars was probably due to this fall of temperature. This idea was ultimately utilised by Sir William Crookes in an interesting variation of the periodic law, in which he assumes that temperature plays a part in bringing about the changes in the characters of the elements. Brodie, in 1880, came to the conclusion that the elements were certainly not elementary, because in what he called a "chemical calculus" he had to assume that certain substances, supposed to be elements, were really not so; and he then threw out the very pregnant idea that possibly in some of the hotter stars some of these elements which he predicted might be found. Nine years afterwards, Rydberg, one of the most industrious investigators of the question of "series" to which I have referred, stated that most of the phenomena of series could be explained by supposing that hydrogen was really the initial element, and that the other substances were really compounds of hydrogen; so that he came back to Prout's first view in 1815. All these ideas imply a continuous action, and suggest that there was some original stuff which was continuously formed into something more complex as time went on. That is to say, that the existence of our chemical elements as we know them does not depend upon their having been separately manufactured, but that they are the result of the working of a general law, as in the case of plants and animals.

It will be gathered from the above statement that the stellar facts are entirely in harmony with the highest chemical thought, and indeed establish the correctness of its major contention. We may be said to pass from chemical speculation to a solid chain of facts, which doubtless will be strengthened and lengthened as time goes on. In all these changes we seem to be in the presence of a series of complications, the possibility of which depends upon a reduction of temperature. There may have been roughly, a series of doublings, or the greater complexities may also have been brought about by the union of different substances. In either case, as temperature falls, we get a possibility of combinations which was not present before; so that more and more complex forms are produced.

In discussing the idea of evolution, both organic and inorganic, we are driven to the consideration of a first form, from which all subsequent ones are derived.

The method of inorganic evolution must depend upon the way in which complications are brought about. Although in this chapter I have dealt with the received chemical view, I shall show subsequently that it is not the only one we have to consider.

It is well to point out that the inquiries referred to in this book are now not the only ones which suggest the evolution of inorganic matter from some primordial element such as I suggested in 1873, to explain the spectroscopic facts then available.

I have already referred to the work recently accomplished on the perturbations of spectral lines. Mr. Preston, in discussing the bearings of his results, thus writes :—*

"We have, I think, reasonable hope that the time is fast approaching when intimate relations, if not identity, will be seen to exist between forms of matter which have heretofore been considered as quite distinct. Important spectroscopic information pointing in this same direction has been gleaned through a long series of observations by Sir Norman Lockyer, on the spectra of the fixed stars, and on the different spectra yielded by the same substance at different temperatures. These observations lend some support to the idea, so long entertained merely as a speculation, that all the various kinds of matter, all the various so-called chemical elements, may be built up in some way of the same fundamental substance."

In the same way Professor J. J. Thomson, in his important investigations of the cathode rays, after describing a new series of facts, writes :—†

"The explanation which seems to me to account in the most simple and straightforward manner for the facts is founded on a view of the constitution of the chemical elements which has been favourably entertained by many chemists: this view is that the atoms of the different chemical elements are different aggregations of atoms of the same kind. In the form in which this hypothesis was enunciated by Prout, the atoms of the different elements were hydrogen atoms; in this precise form the hypothesis is not tenable, but if we substitute for hydrogen some unknown primordial substance X, there is nothing known which is inconsistent with this hypothesis, which is one which has been recently supported by Sir Norman Lockyer, for reasons derived from the study of the stellar spectra."

On these points we must now go more into details.

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* *Nature*, vol. lx, p. 180.

† *Phil. Mag.*, 1897, p. 311.

CHAP. XXII.—THE RELATIONS OF THE ORGANIC AND INORGANIC EVOLUTIONS.

IT may be of interest to briefly consider the processes of inorganic evolution in relation to those of organic evolution. I have already referred to the fundamental difference in the conditions; we found evidence of a running down of temperature which no one can define in the case of the stars; in the case of the organic evolution going on at the present time, we cannot be very much removed from the temperature conditions of the Cambrian formations. That is a point which I have made before, and it is important to insist upon it. Clearly there cannot have been any very great change of temperature during the whole cycle of organic life. Previous to it we have found complexity brought about, possibly by doublings, and certainly by combinations, the result being, as I have already mentioned, more complex forms. Of course, at the dawn of organic life on the surface of the earth there may have been residua of the earlier chemical forms; that is to say, not all the elements which we found in the hottest stars had combined to form the substances of which the earth was composed. However this may have been, although the work of organic evolution, unlike that of inorganic evolution, must have been done under widely different temperature conditions, the result has been the same; it has since provided us with another succession of forms getting more complex as time has gone on, and there is still a residuum of early forms.

We are led, then, to the conclusion that life in its various forms on this planet, now acknowledged to be the work of evolution, was an appendix, as it were, to the work of inorganic evolution carried on in a perfectly different way. Although the way was different, still nature is so parsimonious in her methods—she never does a thing in two ways that can be as well done in one—that I have no doubt that when these matters come to be considered as they are bound to be considered with the progress of our knowledge, we shall find a great number of parallels; but I am not concerned with parallels now. I wish to refer to a chemical point of view which I think of some importance in relation to what has gone before; it is a point which I wish to make depending upon the existence of those elements which make their appearance in the hottest stars.

In inorganic forms, in those represented to us in the hottest stars and the stars of gradually lower temperature, we have forms produced by a method by which complication is brought about; what this method may probably be, we shall consider later on. Now the more of these complications the more the early forms must have disappeared, unless we may take it that they may have been made occasionally to reappear by the destruction of the later forms; that is a point to bear in mind. If the simpler forms must go on combining to provide the more advanced forms, then if all the simpler forms are so used up, the only chance of getting the simpler forms again is to destroy something which had been previously made; and we can quite understand, of course, that there were many conditions of this destruction possible at the time when the crust of the earth was being formed. But however that may be, the gaseous elements, together with the non-gaseous elements first formed, would be the chief chemical substances on the surface and over it. Now the substances over the crust, of course, would be the gases, hydrogen, oxygen, nitrogen, and dealing with the stellar evidence we may suggest carbon combined with them; that is to say, hydrocarbons, carbonic acid, and so on. On the surface, whether the surface be one of land or water, we should expect, in addition to the low melting point metals lithium and sodium, those three metals which we know existed in the hottest stars long before the rest, magnesium, calcium, and silicium. Lithium probably and sodium certainly exist in some of the relatively hot stars; the evidence also suggests sulphur, and this is rendered more probable because of the simplicity of its spectrum-series. Now these are very remarkable associations, and seem far away from ordinary chemical considerations. Is it a mere coincidence that they are the important substances in sea water?

Constituents of Sea-water.

Chloride of sodium	77·75
" magnesium	10·87
Sulphate of "	4·73
" lime	3·60
" potash	2·46
Bromide of magnesium...	0·21
Carbonate of lime	0·34

The most easily thinkable organic evolution under these circumstances would be that of organisms built up of these chemical forms, chiefly because they would represent the more mobile or the more plastic materials; we should not expect organic evolution to have begun in iron, but rather in something the most mobile and the most plastic at the time. The available matter then for this evolution would be those gases plus those metals and those non-metals to which I have referred. Now, supposing such an evolution, if the forms so composed were to be multiplied indefinitely, the available material would be used up and organic evolution would be brought just as certainly to a dead-lock as the inorganic evolution was brought to a dead-lock when there was no possibility of any considerable reduction of temperature. We should expect a tendency to growth among the organic molecules, I dare not call it an inherited tendency, but I feel almost inclined to do so, having the growth of crystals in mind. If when these new organic forms had been produced, the results instead of being stable were emphatically unstable, and still better if a dissolution or the destruction of parts or wholes could be induced, progress would always continue to be possible, and indeed it might be accelerated.*

The new organic molecules would ultimately not have the first user of the chemical forms left available by the inorganic evolution, but they would have the user of the gases and other substances produced by the dissolution of their predecessors. They would be shoddy chemical forms, it is true, but shoddy forms would be better than none. Under these circumstances and in this way, the organic kingdom could go on; in other words, the dissolution of parts or wholes of the new organisms would not merely be an advantage to the race, but might even be an essential condition for its continuance.

It therefore looks very much as if we can really go back as far as these very early stages of life on our planet to apply those lines of Tennyson:—

> "So careful of the type she seems,
> So careless of the single life."

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* My friend and colleague, Professor Howes, has called my attention in this connection to Professor Weismann's views (*Weismann on Heredity*, vol. i, p. 112), who seems to have arrived at somewhat similar conclusions though by a vastly different road. He says, in his *Essay on Life and Death*, "In my opinion life became limited in its duration not because it was contrary to its very nature to be unlimited, but because an unlimited persistence of the individual would be a luxury without a purpose."

The general view I have put forward, however, suggests that perhaps it was not so much a question of *luxury* for the living as one of *necessity* in order that others might live; it was a case of *mors janua vitae*.

The whole question turns upon the presence or absence, in all regions, of an excess of the early chemical forms ready to be used up *in all necessary proportions*. Hence it may turn out that the difficulty was much greater for land- than for sea-forms, that is, that dissolution of parts or wholes of land-forms proceeded with greater rapidity. It is a question of the possibility of continuous assimilation (see Dantec, *La Sexualité*, p. 11), and the word "parts" which I have used refers to the somatic cells, and not to the "immortal" part of living organisms.

We have arrived, then, at a condition in which the same material may be worked up over and over again. In this way ultimately higher forms might be produced. Now, if to this dissolution, as a means of giving us new material, we add reproduction, then we can go a stage very much further. If we take bi-partition, which was the first method of multiplication, as we know, both in the vegetable and animal world, and then obtain a multiplication of forms by halving instead of the inorganic multiplication of forms by complicating, then we can have a very much increased rate of advance.

These, then, roughly, are the ideas touching organic evolution which are suggested by the stellar evidence as to inorganic evolution, and the collocation of the simplest forms noted in the hottest stars.

Let us turn finally to the facts. Biologists are very much more happy than astronomers and chemists, because they can see their units. A chemist professes to believe in nothing which he does not get in a bottle, although I have never yet seen the chemist who was ever happy enough to bottle an atom or a molecule as such; but the superstition still remains with them, and they profess to believe in nothing that they cannot see. Now, the organic cell, the unit of the biologist, is itself a congeries of subordinate entities, as a molecule is made up of its elementary atoms, manifesting the properties common to living matter in all its forms.

The characteristic general feature of the vegetable activity of plant forms is their feeding upon gases and liquids, including sea-water. The progress of research greatly strengthens the view that there was a common life plasm, out of which both the vegetable and the animal kingdoms have developed. Be that as it may, it is found that the vegetable grows upon these chemical forms to which I have referred, and the animal feeds either upon the plant or upon other animals which have in their turn fed upon plants; so that there we get the real chemical structure of the protoplasm, of the real life unit, in our organic evolution.

Here another question arises. Is there any chemical relation between the chemical composition of the organic cell and the reversing layers of the hottest stars—the reversing layer being that part of a star's anatomy by which we define the different genera?

When we study the chemical composition of this cell we find it consists of one or more forms of a complex compound of carbon, hydrogen, oxygen, nitrogen, with water, called protein; and protoplasm, the common basis of vegetable and animal life, is thus composed. This substance is liable to waste and disintegration by oxidation, and there may be a concomitant reintegration of it by the assimilation of new matter.

The marvellous molecular complexity of the so-called simple cell may be gathered from the following formulæ for hæmoglobin:—

Man ... ... $C_{600}H_{960}N_{154}Fe_1S_3O_{179}$.
Horse ... ... $C_{712}H_{1130}N_{214}Fe_1S_2O_{245}$.*

Various different percentage compositions have been given of this protoplasm, but I need not do more than refer to them. It is more important to consider the other chemical substances which go to form it, for there are others besides which it is of interest to study from our stellar point of view. I quote from Mr. Sheridan Lea.†

"Proteids ordinarily leave on ignition a variable quantity of ash. In the case of egg-albumin the principal constituents of the ash are chlorides of *sodium* and potassium, the latter exceeding the former in amount. The remainder consists of *sodium* and potassium, in combination with phosphoric, sulphuric, and *carbonic* acids, and very small quantities of *calcium*, *magnesium*, and iron, in union with the same acids. There may be also a trace of *silica*."

Have we here more coincidences? or is it that the more one inquires into the chemistry of these things the more we are brought back to our stellar point of view, and to the fact that, taking the simplicity of chemical form as determined by the appearance of these different chemical substances in the hottest stars as opposed to the cooler ones, and in relation to the "series" of spectra which they produce, we come to the conclusion that the first organic life was an interaction somehow or other between the undoubted earliest chemical forms? Not only have we hydrogen, oxygen, and nitrogen among the gases common to the organic cell and the hottest stars, but those substances in addition which I have indicated by italics.

Is it possible that we have here a quite new bond between man and the stars?

There is still another point regarding this question of the relation of the two evolutions, inorganic and organic. I refer to the place of organic evolution in regard to inorganic evolution in the scale of time. I do not wish to call too much attention to this diagram, because it is entirely hypothetical; but it is constructed on the simplest principles, so that it shall go as little wrong as may be. I begin by drawing a line at the bottom, to represent the zero of temperature; certain temperature values are indicated on the left-hand side of the diagram. Then we have the assumption that a star loses an equal amount of heat in an equal period of time. In that way, then, at the bottom we have relative times, at the side we have temperatures, in centigrade degrees. Water freezes at a certain temperature above absolute zero, and boils at a certain other point; these are marked on our temperature scale. Then we have to remember that about half way between the boiling point and the freezing point, all

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* *Verworn*, p. 104.

† *The Chemical Bases of the Animal Body*, p. 5.

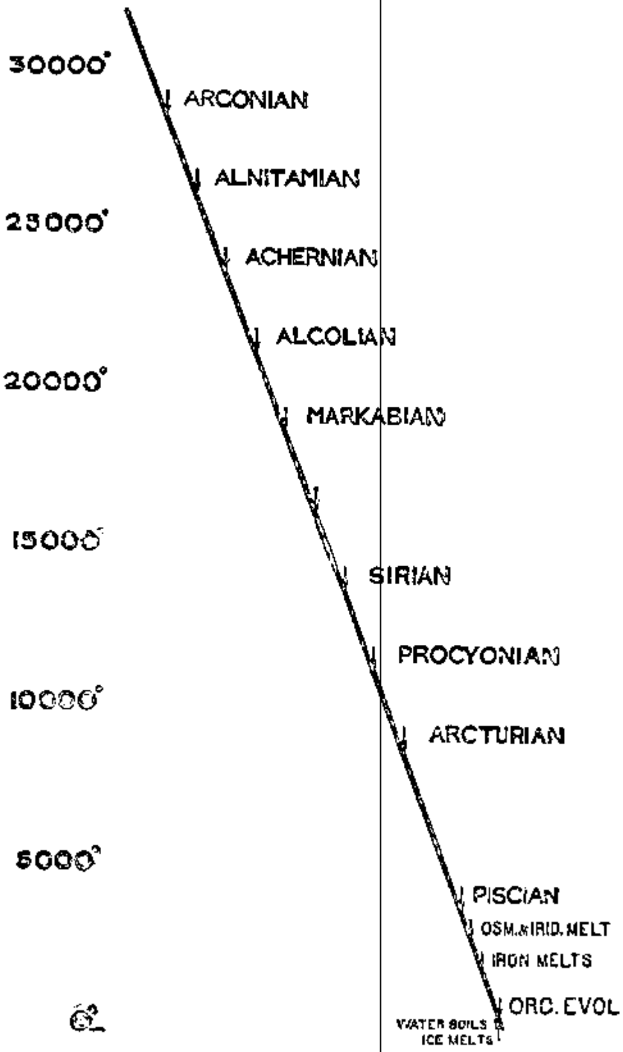


Fig. 44.—Diagram showing that organic evolution occupies only a point in the line representing the time and temperature range required by inorganic evolution.

the organic life with which we are familiar on this planet, from the geological evidence and our own experience, must have gone on at a temperature of somewhere about, let us say, from 50 to 40 C. There, then, we get the limit of organic life in relation to the possible inorganic life represented by the various chemical changes in the stars.

We know from laboratory results that the stars of lowest temperature are about the same temperature as that of the electric arc, which is about 3,500° C., and so we put the Piscian stars there. It has also been stated by Mr. Wilson lately, that the temperature of the sun, measured by several physical methods, is something between 8,000° and 9,000° C., so that we put there the Arcturian stars. Of course we have no means of determining the temperatures of the hotter stars, so I have ventured to make a very modest supposition that possibly we get about half the difference of temperature between those stars as we have found between the Piscian and the Arcturian stars from experiments on the earth. That will give us, roughly, something like 5,000° C. We find, then, that if we assume equal increments of temperature for each of the different genera of stars that I have brought together in Chapter VII, we get a temperature at the top of the diagram of something like 28,000° C. All we have to do, then, is to draw a diagonal line on which to mark the various temperatures considered. On this the organic evolution, which represents everything which has taken place with regard to living forms on the surface of our planet from the pre-Laurentian times to our own, is represented by a small dot.

It looks, therefore, very much as if these recent results of spectrum analysis, may probably be of greater value in the future, because they deal with a multitude of changes and a period of time compared with which all the changes discussed by the geologists are almost invisible on a diagram of this size. Not only shall we have probably some help in determining this scale, but I think that, as I have already indicated, the wonderful similarity between the substances contained in the organic cell and those which would most likely be free when the greatest amount of chemical combination had taken place on the surface of the cooling world, will throw some light on the basis of organic evolution itself.

In this way, then, we have really been only continuing a train of thought, which has to do with Man's Place in Nature, in relation to the Sun's Place in Nature; and finding fresh grounds for thinking that the more different branches of science are studied and allowed to react on each other, the more the oneness of Nature impresses itself upon the mind.

Chap. XXIII.—Inorganic Evolution from a Chemical Standpoint.

In the study of the facts of inorganic evolution presented to us by stellar spectra, there is one point of paramount importance to be inquired into. In the problems of inorganic evolution which we have now to face, it is sufficiently obvious that we have to deal with a continuously increasing complexity of forms, precisely as in organic evolution the biologist has had to deal, and has dealt successfully with, a like increase of complexity of organic forms.

So far the processes by which complexity has been brought about have only been referred to generally; it is time now to endeavour to gain a more detailed insight into the methods by which inorganic complexity has been arrived at. I will discuss this question first in relation to chemical theory.

If we ask the question How has complexity been brought about in the case of known chemical compound bodies? an easy answer is given by analysis. Chloride of sodium, for instance, is thus found to be formed by the combination of chlorine and sodium. But when we wish to deal with the formation of the so-called "elements" themselves, no such easy solution of the question is open to us.

If in order to investigate the problem we take the analogy furnished by compound bodies as our guide, we should say that the molecules of the elements themselves were produced by the combination of unlike forms.

But as a matter of fact, this method of producing complexity is not the only one known to chemists. There are bodies of the same percentage composition which differ in molecular weight; the methane series of hydrocarbons is a case in point; the higher molecular weights, or greater complexes, are produced by additions of the unit CH_2, so that these higher complexes are produced by the combination of similar lower complexes. This process is termed polymerisation.

We are then familiar with two methods of increasing complexity, which we may represent by $a + a$ (polymerisation) and $x + y$ (combination), producing a form A.

This, then, is the problem from the purely chemical side. On which of these methods have the elements themselves been formed, now that we are justified in considering them as compound bodies? I suppose that chemists when hypothetically considering the possible dissociation of the chemical elements would favour the view of depolymerisation; that is, the breaking up of a substance A into finer forms (a) weighed by $A/2$ (or $A/3$), rather than a simplification of A into x and y.

The method of attacking this problem from the chemical point of view in the first instance, must be a somewhat indirect one.

The Stars and the Periodic Law.

In Chap. XXI I referred to the important hypothesis put forward by Newlands, Mendeléef and others in relation to the so-called "periodic law," which law indicates that certain chemical characteristics of the elements are related to their atomic weights.

It will be well now to study this question with a view of discussing it more fully in the light of all the facts known to us, among which the stellar evidence and that afforded by the study of series are, I think, of especial importance; since it may be said that we are now absolutely justified in holding the view that of the lines which make their appearance in the spectra of chemical substances when exposed to relatively high temperatures, a varying proportion is produced *by the constituents of the substance*, whether it be a compound like the chloride of magnesium, to take an instance, or of magnesium itself.

Now the periodic law based upon atomic weights deals with each "element" as it exists at a temperature at which the chemist can handle it; that is, if it be a question, say of magnesium, the chloride or some other compound of the metal must have been broken up, and the chlorine entirely got rid of before the pure magnesium is there to handle, and of this pure magnesium the atomic weight is found, and, having also regard to its chemical characteristics, its position in the periodic system determined.

But if the magnesium be itself compound, the position thus assigned for the element is certain not to tally with the stellar evidence if the temperature of the star from which information relating to it is obtained is high enough to continue the work of dissociation; that is, to break up magnesium itself into its constituents as certainly as the chloride of magnesium was broken up in the laboratory in the first instance.

It is now known that dealing with this very substance magnesium, high electric tension brings us in presence of a spectrum which consists of at least two sets of lines, numerous ones seen also at the temperature of the arc, and a very restricted number which make their appearance in the spark.

If this be the work of dissociation—and, as I have shown, the proofs are overwhelming—the "atomic weight" of the particle, mole cule or mass, call it what we will, which produces the restricted number of lines—the enhanced lines—must be less than that of the magnesium by the breaking up of which it is brought into a separate existence.

And now comes the chief point in relation to the periodic law. *Seeing that the smaller masses which produce the enhanced lines have not been yet isolated, their "atomic" weights and their chemical characteristics have not been determined, and so of course their places in the periodic table cannot be indicated as it at present exists.*

My contention, therefore, is that some, at all events, of the apparent discrepancies—for there are discrepancies—between the stellar evidence and the "periodic" hypothesis arise from this cause.

The magnesium, and I will now add calcium, which the chemist studies at relatively low temperatures have atomic weights of 24 and 40 respectively, and the stellar evidence would be in harmony with the periodic law if magnesium (24) made its appearance after sodium (23), and calcium (40) after chlorine (39), and generally each substance should make its appearance after all other substances of lower atomic weight than itself.

But, and again for the sake of simplicity I shall confine myself to magnesium and calcium for the moment, in the stars we find lines in the high temperature spectrum of magnesium and calcium appearing before known lines in the spectrum of oxygen which has an atomic weight of 16.

How are these results to be reconciled? I suggest that the explanation is that the substances revealed by the enhanced lines of magnesium and calcium and noted in the hottest stars have lower atomic weights (smaller masses) than the oxygen of the periodic table.

Let us next, then, see what these atomic weights may possibly be. Assuming A 2, the atomic weight of proto-magnesium would be 24 2 = 12; of proto-calcium 40 2 = 20, supposing only one depolymerisation has taken place. If we assume two depolymerisations, we get 6 and 10 as the "atomic" weights of the simpler forms of magnesium and calcium which make their appearance in the hottest stars.

In this way we can explain the appearance of those finer forms of magnesium and calcium before oxygen, with a small number of depolymerisations, and the stellar record of the order of atomic weights would be the same:—

Hydrogen	1
Proto-calcium	10
Proto-magnesium	12
Oxygen	16

So much, then, for a possible reconciliation. The next point to be considered is, is depolymerisation on such a small scale sufficient?

To do this we have to see the basis of the atomic weight of oxygen 16, and consider the series question in relation to oxygen. This necessitates a reference back to Chapter X, in which I pointed out that the simplest case presented in series phenomena is that placed before us by sodium and other elements which run through all their known spectral changes at a low temperature. Dealing with the line spectrum stage we have three "series," one principal and two subordinate (first and second). The former contains the orange line D, constantly seen at all temperatures, the first subordinate the red line, the second subordinate the green line, representatives of two series of lines which are best seen both in the flame and arc.

The two subordinate series of sodium, like those of all other elements so far examined, have the peculiarity that they end at nearly the same wave-length, while the end of the principal series occurs at a different, sometimes widely different, wave-length. This is a touchstone of the highest importance, as we shall see; it points to a solidarity of the two subordinate series, and to a difference between them and the principal series.

Although the original idea was that all three series were produced by the vibrations of the same molecule, observations of the sodium phenomena alone are simply and sufficiently explained by supposing that we have three different masses vibrating, and that two of them, producing the subordinate series, can be broken up by heat, while that producing the principal series cannot. The series represented by the red and green lines seen best at the lower temperatures have been seen alone, and it is a matter of common experience that the yellow line representing the principal series is generally seen alone; it is not abolished at high temperature as the others are. Because the mass, the vibrations of which give us the yellow lines, is produced by the breaking up of more complex forms at a low stage of heat, and it cannot be destroyed by the means at our command, it is the common representative of the element sodium. Because the masses, the vibrations of which produce the two subordinate series represented by the red and green lines, are easily destroyed by heat, they are more rarely seen; scarcely ever at high temperatures when the quantity is small, since, as I pointed out years ago, "the more there is to dissociate, the more time is required to run through the series, and the better the first stages are seen."

This view is greatly strengthened by considering another substance which, if we accept Pickering's and Rydberg's results, has like sodium, three series, one principal and two subordinates in quite orthodox fashion. I refer to hydrogen. The facts concerning which are given on p. 95.

Till a short time ago we only knew of one "series" of hydrogen, and on this ground Rydberg assumed it to represent the finest form of matter known, regarding the other substances which give three normal series as more complex. This idea is in harmony with the view expressed above.

If we accept the recent suggestions, we must regard hydrogen as identical with sodium in its series conditions. But there is this tremendous difference. In sodium we easily at low temperatures—the Bunsen is sufficient—see all three series, while in the case of hydrogen even the Spottiswoode coil can show us nothing more than one of the subordinate series. At the same time, the other subordinate and the principal series are visible in stars which we have many reasons for believing to be hotter than the spark produced by the Spottiswoode coil.

The argument for the existence of at least three different masses producing the three different series, derived from the sodium observations, is therefore greatly strengthened by what we now know of hydrogen.

I shall therefore assume it in what follows, which has reference to more complicated phenomena.

Oxygen, instead of having three series like metals of low melting point such as sodium, and the gas hydrogen, has *six*. These six have been divided by Runge and Paschen into two normal sets of three, each set possessing one principal and two subordinate series.

There is evidently a new problem before us; we require to add the series of hydrogen to the series of sodium to get a "series" result similar to that obtained from oxygen.

Before we go further it will be well to consider the possible order of simplifications. Let us take the simplest case represented by sodium and hydrogen in the first instance. The facts are shown in the following table:—

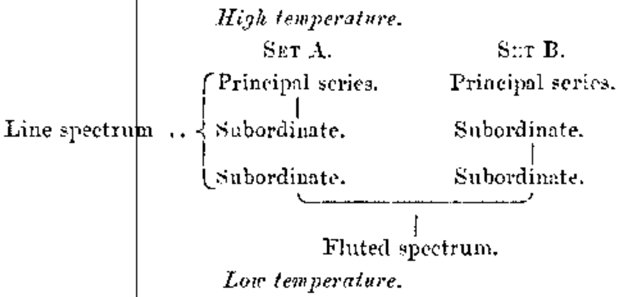
High temperature.

Sodium.			*Hydrogen.*		
Line stage	Principal Subordinate	Celestial and terrestrial vapour.	Line stage	Principal Subordinate	Celestial gas.
	Subordinate	Terrestrial vapour.		Subordinate	Terrestrial gas.
Flutings	.. ..		Structure spectrum	..	
Continuous	.. ..	Solid and liquid.	Continuous	.. ..	

Low temperature.

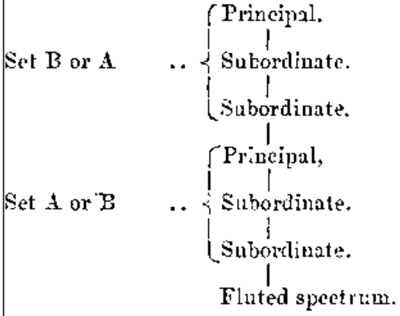
We may now bring these results to bear upon oxygen. We learned first from Egeroff that this gas at ordinary temperature and pressure is so molecularly constituted that it produces a fluted absorption in the red part of the spectrum. On account of the constancy of the results obtained by chemists we cannot be dealing with a mixture of molecules, the fluting absorption therefore must be produced by molecules of one complexity having an "atomic weight" of 16.

If we subject it to an induced current at low pressure (at which the action of such a current is feeblest), it at once breaks up into two normal sets of three series, that is six series altogether; it is almost impossible to consider this state of things in the light of what happens in the case of sodium and hydrogen without assuming on the ordinary chemical view that the "molecule" with the fluted spectrum is broken up into two, until finally we get—



But if we accept this, we give up depolymerisation, for the molecules of the subordinate series of sets A and B thus produced cannot be identical, because their spectra are not identical.

If we hold to depolymerisation we must arrange matters thus—



and we get six depolymerisations.

The number of lines measured by Runge and Paschen in the spectrum of oxygen at low temperature was 76; of these the six series referred to contain 56, leaving 20 residual lines. Now if we employ a strong induced current at atmospheric pressure, we practically extinguish these six series of lines and produce a new spectrum altogether, containing a still greater number of lines: 114, according to Neovius. Only one line is common to his table and that of Runge and Paschen. About the series conditioning of these new lines we are at present profoundly ignorant.

Let us take the simplest course in harmony with the principle of continuity, and suppose that the great number of new lines is due to the breaking up of the molecules of the upper principal series given in the previous table into representatives of a still finer form, as hydrogen, as we know it, is broken up into a finer form at the highest stellar temperatures.

Have we, on the line of reasoning we are pursuing, any means of estimating the number of finer forms which may be at work to produce the 113 new lines?

One possible way—a statistical way—seems open to us. Taking the number of lines already recorded in the spectra roughly between λ 7000 and λ 2600 of the following substances, which give us three series—lithium, sodium, potassium, helium, asterium, hydrogen—we find that the number of lines in each series and the total numbers are as follows:—

	Maximum number.	Minimum number.	Average number.
Principal series ..	10 Ast	1 H	7
First subordinate ..	17 He	6 Na	9
Second subordinate ..	12 He	4 Li	8
Totals	39	11	24

This indicates that in oxygen we are slightly above the average with $\frac{56}{2} = 28$ lines per set. If we take the facts for oxygen itself, which give us 56 lines for two sets of three, the 113 lines will give almost exactly four additional sets of three series, and therefore the possibility of twelve more depolymerisations if this method of simplification is considered.

Of course we can halve the number of depolymerisations by assuming that the fluting molecule instead of being depolymerised is broken up into x and y, the bases of the two systems of series.

Now it is this last crop of new lines alone which is represented in the hottest stars, and no one, I think, will now urge that some kind of simplification which may include depolymerisation has not taken place before they were brought into evidence.

Our base of 16 then vanishes, and with it the previously considered possible atomic weights of the forms of magnesium and calcium which precede the appearance of oxygen in the hottest stars. We must therefore assume further depolymerisations in the case of these metals beyond those considered in the first instance.

I now come to another point. How do the above considerations bear upon hydrogen with its atomic weight of 1? Of this hydrogen we know nothing spectroscopically. There is evidence that it is broken up into something which gives the complicated structure spectrum with hundreds of lines not yet sorted into series, again into the one series seen in our laboratories and in the cooler stars, still again into two other forms we cannot get here.

Let us apply the statistical method we employed in the case of oxygen.

In the region included in these inquiries the number of hydrogen lines in the three series referred to is 17. Hasselberg has measured 454 lines in the structure spectrum between λλ 642 and 441. Now if this spectrum is built up of series similar to those observed at the highest temperatures, we must have more (seeing that Hasselberg's work was limited) than $\frac{454}{17} = \pm 27$ series or 9 sets of 3 each. We deal then altogether with 12 depolymerisations.

But to be on the safe side, let us assume 6 on the ground that the lines in the series may be more numerous, and that some of Hasselberg's lines may be due to flutings. It will be clear that the masses or "atomic weights" we arrive at must be very small. Here is the story:—

Spectrum.	Where existent.	Series, &c.		Mass.
Line spectrum ..	Celestial	Principal	..	0·0019
		Subordinate	..	0·0039
	Terrestrial	Subordinate	..	0·0078
Fluted spectrum ..	Set B Terrestrial	Principal	..	0·0156
		Subordinate	..	0·0312
		Subordinate	..	0·0625
	Set A Terrestrial	Principal	..	0·125
		Subordinate	..	0·25
		Subordinate	..	0·5
Continuous spectrum		Hydrogen weighed in the cold	..	1

Such a conclusion as this, and therefore the reasoning which has led up to it, must stand or fall according as science knows anything of such masses.

I shall show subsequently that, thanks to the investigations of Prof. J. J. Thomson, science is beginning to know a great deal of such masses, and the result of this work may therefore favour the view that polymerisation is a *vera causa* for molecular complexity, at all events in the cases of elements of low atomic weight; if we accept the ordinary chemical view.

Let us then consider the case of those elements the atomic weight of which is greater. In the first stages of evolution, in which we deal with substances of relatively low atomic weight, the stellar evidence supplies us with definite landmarks, and these are definite because the spectra of the hottest stars are not overcrowded with lines. After we have passed the gaseous and proto-metallic stages, however, we find the spectra full of lines which we see at the temperature of the arc, and metals of relatively high atomic weight and melting point are involved; the exact sequences are naturally more difficult to follow, and therefore the *method* of evolution may escape us.

Kayser and Runge have shown that the melting point has a profound influence on the "series" conditions. Those with the highest melting points, such as barium and gold, present us with no series. There is generally such a flood of lines that it has been so far impossible to disentangle them; we have the "structure spectrum" of hydrogen repeated in these metals at arc temperatures in the so-called "arc spectrum."

I have already said that I think most chemists would consider that the formation of larger masses by polymerisation is more probable than by the coming together of dissimilar atoms; but if we consider chemical compounds, certainly the analogy is all in favour of the latter view if the principle of continuity be taken into account, for we are ignorant of the point at which one evolutionary process resigns in favour of another. The present separation of compound from simple bodies is, indeed, simply a measure of our ignorance arising from the feebleness of our laboratory resources in relation to the temperature required to produce more and more simplifications.

I discussed the question in my *Chemistry of the Sun* in 1887, and showed that the analogy of the completely studied hydrocarbon series beginning with CH_2 suggested a hypothetical elemental sequence.

$a \quad b$, separate.
$a + b$, combined.
$a + (b + b)$, written by chemists ab_2.
$a + (b_2)\ (b_2)$ „ „ ab_4.

and so on.

In the concrete hydrocarbon series we have continuous additions of CH_2 to CH_4 until we reach a molecule defined by $C_{16}H_{34}$, and as the building up of this molecule can be traced without difficulty, so we can imagine it simplified by successive *sheddings* of its constituent CH_2; we pass from a simplification which we can bring about by simple halving to one which provides us with relatively large and small masses.

Chap. XXIV.—Inorganic Evolution from a Physical Standpoint.

The next question which arises is whether there is any way open to us of getting still more light on this matter beyond that furnished by orthodox chemistry.

With the progress of science the idea of "atoms" has considerably changed.

Formerly they were regarded as merely chemically different from element to element; the recent investigations have introduced a new conception. It is now no longer chemically different matter merely, but matter, whether chemically different or not, *carrying an electric charge*. In the first work along this new line, physicists, in order to grapple with the phenomena of electrolysis and solutions, imagined sub-molecules or sub-atoms carrying an electric charge in an electrolyte from the anode to the cathode; this was called an ion (Gr. a goer). This conception has been more recently used to explain those movements of particles of matter which produce light, and therefore spectral lines. The sub-particle, this *ion*, with its electric charge e and its mass m, is supposed to move in an elliptic orbit under the attraction of a centre. At first the theory supposed the ions to be electrified particles, but a recent extension considers them to be complex dynamical systems, the motions of which are registered by spectral phenomena.

It will be gathered from what I have already said relating to the various questions connected with the study of "series" of spectral lines how the idea of "complex dynamical systems" is also demanded to explain the phenomena presented by them.

Thus I have shown it to be probable that the hydrogen atom which the chemist weighs may be built up of hundreds of the things, call them what you will, a few of which in the hottest stars produce the vibrations which we take as demonstrating the existence of hydrogen in the celestial spaces.

Both these lines of modern evidence tend to justify the view that the different spectra are not produced by different material, but by different conditionings of the same material.

These different conditionings may refer either to the electric charge or to the mass of the ion, or of the molecule round which the ion circulates. The units of matter present in the ion or in the central molecule may vary in number, or their arrangement may vary.

Imagine a series of substances "chemically" different, the intrinsic difference of which, from A the simplest to Z the most complex, really consists merely of their being built up of *different numbers of units*. When Z is simplified by heat, its complex system of centre of force and ion with their electric charges will undergo changes which we may expect to result in the formation of less complex systems doubtless built on a like pattern, and therefore capable of producing spectra; hence we are bound to see the spectra of some of the intermediate forms which, when they are stable and go about in company, it may well be that physicists have already recognised. These we may call B or C, or R or S, or X or Y, as representatives of various complexities.

The more complex the form experimented on and the higher the temperature employed in the laboratory, the more spectral lines indicating different chemical "elements" in intermediate stages may we see.

I say in the laboratory, because in the stars the result will be different. There, in consequence of the long continued action of heat and the shielding of the reversing layer from the effects of lower temperature, we may only see at the highest temperature the spectra of the forms A and near A. We now know what these are.

To take another case; let us assume that the electric charges or arrangement, as well as the number of the units of matter, may vary. Under these conditions, when we dissociate Z, not all, but only some, of possible intermediate forms may be expected to afford spectral evidence. Say, to take an example, those in the vertical columns of Mendeléef's table; and I am led to make this suggestion, because Kayser has shown that in "series" the duplicity or triplicity of lines is associated with the position of the elements producing them in these columns. A concrete case would be afforded by contrasting the behaviour of sodium and cæsium, representing relatively simple and complex substances. We might observe the lines of sodium when cæsium is dissociated; we should not expect to see the lines of cæsium when sodium is dissociated.

The two cases taken it is possible may illustrate the difference between related and not related groups of "elements."

The apparently constant appearance of representative lines of the spectrum ot one substance of a group in that of the other members of the same group may be thus explained, although it has generally been attributed to the presence of impurities, as in the case of all common long lines seen in spectra; and this in spite of the protest that if the purest specimens known (I have worked on beads of Stas' silver which had never been touched) were so impure, some of the decimals used to express their atomic weight might be well spared. But it is not a question of apparent impurities only.

It is possible that some of the gases of lower atomic weight which exist in the hottest stars may be represented by A in opposition to heavy metals represented by Z, the existence of which is known in the cooler stars only.

The giving off of gases from metals when high tension electricity is employed is well known. This has been explained by assuming them to be "furnace gases," that is gases "occluded" by the metals during their reduction. But this does not seem to be a sufficient explanation, for the same gases are given off by meteorites. We now see why something like this may happen if there is any foundation for the modern conception of the structure of the "atom"; and do not these facts explain the chemistry of the hottest stars?

It is too early yet to attempt to discuss the effects of the electric charge in this connection, but it must be pointed out that so soon as the ions, however associated their units may be, which are supposed always to have an electric charge upon them, are subjected to the action of a voltaic or induced current, the spectral phenomena observed when they are heated are liable to great changes in some cases, and especially when high atomic weights are in question. Doubtless we have here a field of research which will ultimately supply us with the most precious knowledge. I have already shown that with the gases, such as hydrogen and oxygen, heat alone gives rise to no spectral phenomena, while in the case of such metals as sodium, heat is so effective in its dissociating power that the subsequent application of electricity produces no further change.

We have, in fact, to consider that the effects produced on different substances under the same conditions may be different, and that the stars carry us further than our laboratories; that is, there are stages of spectral change within and beyond our experimental powers revealing a *shedding* of ions or some rearrangement of material at different temperatures. Of course it is possible that the rearrangement of material may take place in the central molecule itself; the point to be remembered is, that whatever may happen, whether in the central molecule or the ion, a higher temperature will be associated with a simplification of the total mechanism.

Dr. Preston's Researches.

Quite recently the study of magnetic perturbations of spectral lines has brought a fresh array of evidence on this question.

It has now been proved that spectral phenomena are different when the light source under examination is subjected to the action of a strong magnetic field which, among other things, causes a precessional movement of the orbits of the ions to which I have already referred.

In order to consider the bearing of this, let us deal with the spectrum of zinc which contains triplets. It has been shown that denoting these in ascending order of refrangibility by A_1, B_1, C_1, A_2, B_2, C_2, &c., the lines A_1, A_2, &c., show the same magnetic effect in character, and have the same value of e/m. The lines B_1, B_2, B_3, &c., and C_1, C_2, C_3, &c., form other series, and possess a common value for the quantity $e\,m$ in each case.

Dr. Preston, one of the most successful workers in this new field, states:—

"The value of $e\,m$ for the A series differs from that possessed by the B series, or the C series, and this leads us to infer that the atom of zinc is built up of ions which differ from each other in the value of the quantity $e\,m$, that each of these different ions is effective in producing a certain series of lines in the spectrum of the metal."

But this is by no means all that is to be learned from Dr. Preston's researches. He writes:—

"When we examine the spectrum of cadmium or of magnesium—that is, when we examine the spectra of other metals of the same chemical group—we find that not only are the spectra homologous, not only do the lines group themselves in similar groups, but we find in addition that the corresponding lines of the different spectra are similarly affected by the magnetic field. And further, not only is the character of the magnetic effect the same for the corresponding lines of the different metals of the same chemical group, but the actual magnitude of the resolution, as measured by the quantity $e\,m$, is the same for the corresponding series of lines in the different spectra. This is illustrated in the following table, and leads us to believe, or at least to suspect, that the ion which produces the lines A_1, A_2, A_3, &c., in the spectrum of zinc is the same as that which produces the corresponding series A_1, A_2, A_3, &c., in cadmium, and the same for the corresponding sets in the other metals of this chemical group. In other words, we are led to suspect that, not only is the atom a complex composed of an association of different ions, but that the atoms of those substances which lie in the same chemical group are perhaps built up from the same kind of ions, or at least from ions which possess the same $e\,m$, and that the differences which exist in the materials thus constituted arise more from the manner of association of the ions in the atom than from differences in the fundamental character of the ions which build up the atoms."

Magnetic effect.	Nonets or complex triplets.	Sextets.	Triplets.
Cadmium .. .. $\lambda =$	5086	4800	4678
Zinc .. .. .. $\lambda =$	4811	4722	4680
Magnesium .. .. $\lambda =$	5184	5173	5167
Precessional spin	$e/m = 55$	$e/m = 87$	$e/m = 100$

[This table shows the effect for the three lines which form the first natural triplet in the spectrum of cadmium compared with the corresponding lines in the spectrum of zinc and magnesium. It will be seen that the corresponding lines in the different spectra suffer the same magnetic effect both in character and magnitude. Thus the corresponding lines 4800, 4722, 5173 are each resolved into sextets, and the rate at which the ionic orbit is caused to precess is the same for each (denoted by $e/m = 87$ in the table). Similarly for the other corresponding lines.]

This is a result of the first order of importance. I previously discussed what might be expected to happen if the complex system giving the spectrum of an element were *broken up,* and showed that if less complex systems of the same pattern—that is, consisting of centre of force and ion with its electric charge—were thus produced, these systems would be just as capable of giving spectra as the one the breaking up of which produced them. We should get new ions free to move and vibrate, and new spectra which may reveal the constituents, that is, the manner in which the complex system breaks up. But Dr. Preston goes further that this. He shows that the same ion associated with different centres of force gives us lines at different wave-lengths. That a certain ion which in the spectrum of magnesium gives rise to *b* is also present in zinc and cadmium, though there is no trace of *b* in their spectra.

Now, if the views held by those who have worked along any of these lines be confirmed, we shall be compelled not only to give up polymerisation as the only cause of greater complexity of the molecules of the elements, but to acknowledge a great strengthening of the view that all chemical atoms have a common basis, and build new mental images on this basis. I now pass from the spectroscopic evidence to work in a new field.

Professor J. J. Thomson's Researches.

I have before referred to the fact that science now has to consider masses much smaller than the atom of hydrogen. This we owe not only to a discussion of the phenomena of series, but also to some recent researches of Professor J. J. Thomson, made in connection with his work on the cathode rays.

Since the cathode rays produce luminous effects their path can be traced, hence it is known that they are deflected in a magnetic field. This deflection depends upon the mass of each particle and the electric charge it carries, that is, upon their ratio, m/e. This ratio Professor J. J. Thomson finds to be about $\frac{1}{700}$th of the corresponding value for the hydrogen ion in ordinary electrolysis.

At the same time it has been found by Professor J. J. Thompson and Mr. Townsend that the electric charge e is the same for cathode rays and a hydrogen ion. The m/e in fact may be regarded as independent of the nature of the gas. Since then the m/e of the hydrogen ion is 700 times greater than in the case of cathode particles, the m, the smallest mass whose existence Professor J. J. Thomson has glimpsed, can only be about $\frac{1}{700}$th of the hydrogen ion.

Professor J. J. Thomson writes:—[1]

"The explanation which seems to me to account in the most simple and straightforward manner for the facts is founded on a view of the constitution of the chemical elements which has been favourably entertained by many chemists; this view is that the atoms of the different chemical elements are different aggregations of atoms of the same kind.

* * * * * *

"Thus on this view we have in the cathode rays matter in a new state, a state in which the subdivision of matter is carried very much further than in the ordinary gaseous state: a state in which all matter—that is, matter derived from different sources, such as hydrogen, oxygen, &c.—is of one and the same kind, this matter being the substance from which all the chemical elements are built up.

* * * * * *

"The smallness of the value m/e is, I think, due to the largeness of e as well as the smallness of m. There seems to me to be some evidence that the charges carried by the corpuscles in the atom are large compared with those carried by the ions of an electrolyte."

Thus the whole question of dissociation has been advanced because while on the chemical view we have to deal with intrinsically different kinds of matter from element to element, on the view of Professor J. J. Thomson m is a constant for every element, reminding one of Rydberg's general formula for series in which N_0 is practically a constant for every element, although Rydberg acknowledges slight variations which may be due to errors of observation.

---

[1] *Phil. Mag.*, vol. xliv, p. 311, October, 1897.

Professor J. J. Thomson is thus led to the following view of the differences of construction of a simple "atom" and a compound "molecule":—

"In the molecule of HCl, for example, I picture the components of the hydrogen atoms as held together by a great number of tubes of electrostatic force; the components of the chlorine atom are similarly held together, while only one stray tube binds the hydrogen atom to the chlorine atom."

Dr. Preston's results on the magnetic perturbation of lines, to which I have already referred, leads him to the same general conclusions as those arrived at by Professor J. J. Thomson in favour of the view of dissociation. He says:—

"It may be, indeed, that all ions are fundamentally the same, and that differences in the value of e/m, or in the character of the vibrations emitted by them, or in the spectral lines produced by them, may really arise from the manner in which they are associated together in building up the atom."

The Three Ways of Inorganic Evolution.

At the present time, then, we have before us three suggested ways of inorganic evolution.

Taking the chemical view, this may depend on

(1) Polymerisation, or the combination of similar chemical molecules; or

(2) The combination of dissimilar chemical molecules.

In the new physical view all this is changed into

(3) The gradual building up of physical complexes from similar particles associated with the presence of electricity.

In this last conception we have the material world, up to the highest complex, built up of the same matter under the same laws; as in spectrum analysis there is no special abrupt change between the phenomena presented by the simple and compound bodies of the chemist, so also in the new view there is no break in the order of material evolution from end to end. I have already, on p. 167, referred to the opinions expressed by Professor J. J. Thomson and Dr. Preston, as to the manner in which the new work supports my view expressed many years ago.

Certainly the new view seems competent to throw light on many facts which lacked explanation on the old one, by whatever method of evolution the higher complexes were assumed to be brought about, because on the ionic theory we can imagine several first forms, so that the question of *descent* comes later with the introduction of more complex systems. These various first forms bring about the possibility of evolution along several parallel lines, as well as of the possibility of an infinite number of intercrossings. In this connection we must not forget that the constituents of the reversing layer of Bellatrix and of protoplasm are nearly identical, while the particular forms of matter of which they are composed make so little show in the sun.

A consideration of the central congeries of material units and the ion revolving round it, suggests that the ion may be the more constant in its structure, and that it is to a large extent to the varying mass and charge representing the centre of force that spectral changes are due. It may be that the subordinate "series" indicate that very small variations of complexity are possible, as well as greater ones.

The ions visible in the simple spectra of the hottest stars may be those associated with the smallest centres of force. These are, so far as we know at present, hydrogen, helium, asterium, oxygen and nitrogen among the gases; carbon and silicium, and calcium, magnesium, and sodium among the metals in the forms we study by their spectra at the highest temperatures we can employ in our laboratories.

As the stars cool larger aggregates of material units in the centres of force round which these ions revolve become possible, and hence the complexity of the spectrum of uranium and of the sun, representing a cool star, are both explained by the same process, the various stages of which can be reproduced in the reverse direction by various degrees of dissociation.

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